# Essays on political economy of finance and fintech

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# Essays on Political Economy of Finance and FinTech

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## Introduction

This thesis consists of two chapters in political economy of finance and one chapter in FinTech. My central interest is to study the interaction between socioeconomic stability and financial activities of corporations and financial institutions. The first chapter focuses on whether economic shocks trigger labour unrest and fuel political extremism. The second chapter provides an analysis as to how state-owned firms use internal funds to address sudden social unrest events. The final chapter investigates if new peer-to-peer (P2P) lending technology undermines macroprudential regulation and adds risk to financial stability.

In Chapter 1, we aim to answer a long existing question involving economics, political science, and history: do economic shocks trigger social unrest? The literature lacks a clear answer to this question, primarily because causality runs both ways: social unrest can both disturb labour relations and worsen investment opportunities, thus leading to reductions in economic output. To address this empirical challenge, we go back to the 1930s in China, where the 1933 U.S. Silver Purchase Program acts as a shock to bank lending. Using hand-collected loan-level data, as well as detailed labour unrest and communist penetration information, we document how the silver purchase shock results in a severe credit contraction, and that firms borrowing from banks with a larger exposure to the shock experience increased labour unrest and Communist Party penetration among their workers. This study contributes to the understanding of social consequences caused by credit contraction.

In Chapter 2, I study how social instability affects firm behaviour. Particularly, I show that state-owned enterprises (SOEs) can be used as a political mechanism for the government to maintain social stability. Upon major social unrest events, SOEs strategically allocate internal funds within their business groups to offset local instability by altering labour payments, hiring, and investment levels. To pursue the political goal of maintaining stability, SOEs lose market value, but their allocation strategy helps recover local sentiment after unrest occurs. Additionally, I find that recovering social stability is associated with improved long-term firm performance. This study contributes to the literature by showing how the intra-group allocation of resources incorporates political objectives and yields socioeconomic impact.

In the final chapter, we study to what extent P2P lending poses a threat to loan-to-value (LTV)- based regulation on the traditional credit market. With the growing popularity of P2P

lending, concerns arise as unregulated P2P lending could potentially help borrowers circumvent LTV caps, which aim to prevent excessive household leverage. We use a policy shock that tightened mortgage LTV caps in a number of Chinese cities in 2013 to study whether borrowers borrow more from P2P platforms in treated cities to elude credit restrictions. Relying on transaction data from Renrendai, a major Chinese P2P platform, we show that P2P loans increase in treated cities. However, lenders do not adjust screening strategies around the shock, despite the fact that the increased loans default more ex-post. This finding indicates that without proper regulation, P2P lending can allow risker borrowers to circumvent traditional LTV-based credit restrictions, thus add threat to financial stability.

# Contents

**Introduction** ..... **5**

**1 Credit and Social Unrest: Evidence from 1930s China** ..... **9**

1.1. Introduction ..... 10

1.2. Historical background ..... 14

1.3. Data ..... 18

1.4. Empirical analysis and results ..... 22

1.5. Alternative explanations and discussion ..... 29

1.6. Conclusion ..... 34

Figures and tables ..... 35

Appendix 1.A. ..... 53

Appendix 1.B. ..... 58

Appendix 1.C. ..... 60

Appendix 1.D. ..... 70

**2 Social Stability and Resource Allocation within Business Groups** ..... **74**

2.1. Introduction ..... 75

2.2. Theoretical framework ..... 80

2.3. Institutional background ..... 82

2.4. Data ..... 85

2.5. Empirical analysis and results ..... 87

2.6. Additional tests ..... 95

2.7. Conclusion ..... 99

Figures and tables ..... 100

Appendix 2.A. ..... 110

Appendix 2.B. ..... 112

**3 Can Technology Undermine Macroprudential Regulation? ..... 119**

3.1. Introduction ..... 120

3.2. Predicted impact of the change in LTV caps ..... 125

3.3. Data and identification ..... 128

3.4. Baseline tests ..... 133

3.5. Other loan features; loan performance ..... 136

3.6. Evidence on the 2015 episode ..... 138

3.7. Discussion and policy implications ..... 140

3.8. Conclusion ..... 140

Figures and tables ..... 142

Appendix 3 ..... 152

**Bibliography ..... 154**

# Chapter 1

## Credit and Social Unrest: Evidence from 1930s China

**Abstract**

Do economic shocks trigger social unrest? To answer this question, we turn to a natural experiment in 1930s China, where the 1933 U.S. Silver Purchase program acts as a shock to bank lending. This setting eliminates potential confounding effects of policy, focuses the set of relevant social actors (factory workers and the Communist Party), and provides an exogenous shock to credit, limiting the scope for reverse causality. We assemble a novel, hand-collected dataset of loan contracts between banks and individual firms, labor unrest episodes, and underground Communist Party penetration. We show that the Silver Purchase shock results in a severe credit contraction, and that firms borrowing from banks with a larger exposure to the shock experience increased labor unrest and Communist Party penetration among their workers. These findings contribute to understanding the socio-political consequences of credit (and more in general, economic) shocks.

JEL: G01, G21, N15, N25.

Keywords: Silver Purchase program, bank lending, social unrest, financial history.

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## 1.1. Introduction

Do economic shocks trigger social unrest? Versions of this question often feature in the academic debate in economics, political science, and history, among policy makers, as well as in the general public. Examples include whether tight money and credit led to the development of populist movements in 19th century United States (Friedman and Schwartz (1963), pp. 116-117; Rodrik (2017)); whether the Great Depression drove the Nazis to power in 1930s Germany (Feinstein, Temin, and Toniolo (1997), pp. 120-124); and more recently, whether the Eurozone crisis of 2010-11 fueled mass protests and the rise of populist movements in southern Europe.[1] The intensification of social unrest in a number of countries in recent years has also led to renewed interest in its causes and drivers by international organizations such as the OECD (Renn, Jovanovic, and Schroter (2011)), the World Bank (Word Bank (2016)), and the International Labor Organization (ILO (2013)).

Despite its relevance, however, the question does not yet have a clear answer in the literature. That is because a test of the relationship between the economy and the emergence of social unrest poses at least three challenges.[2] First, causality can run both ways (Alesina and Perotti (1994)): social unrest itself can exacerbate labor relations (Kennan (1986)) or worsen investment prospects (Blattman and Miguel (2010)), thus reducing employment and output. As a result, it is difficult to determine whether social unrest precedes or follows aggregate movements in the economy, as well as to design policy responses (Renn, Jovanovic, and Schroter (2011)). Second, inference is typically confounded by the presence of fiscal and monetary authorities, whose policies may pursue social objectives.[3] Third, present-day social unrest has been associated with movements outside the political mainstream, whose ideological base is often vaguely delineated.

[1] See, for instance, "Populism: What Happens Next?" Financial Times, 9 January 2015.

[2] We define social unrest as a state of generalized dissatisfaction that gives rise to public disturbances, such as riots, strikes, and public rallies, as well as increasing support for movements outside mainstream political ideologies. The expression "social unrest" is consistent with usage by policy institutions and international organizations. Our definition embodies the focus by such institutions on labor market conditions, standards of living, and trust in the government (see Renn, Jovanovic, and Schroter (2011) and ILO (2013)).

[3] For instance, the Federal Reserve is charged with conducting monetary policy "so as to promote effectively the goals of maximum employment" together with "stable prices, and moderate long-term interest rates." See the Federal Reserve Reform Act of 1977 and the Full Employment and Balanced Growth Act of 1978. Passarelli and Tabellini (2017) provide a general theory of how political unrest may affect government policies.

It is thus hard to trace a fringe movement's support to a specific social class exposed to economic hardship.[4]

Our study addresses these challenges, studying the effects of a credit market shock in 1930s China. First, we isolate the direction of causality via a natural experiment, triggered by the U.S. 1933 Silver Purchase program. Undertaken for purely U.S. domestic reasons, and independent of Chinese economic conditions, the Silver Purchase raised the price of silver worldwide and drained the Chinese silver stock. Because China was on the silver standard, the credit capacity of its banks was tied to their silver reserves; we thus use the Silver Purchase shock to identify changes in credit supply. Second, in our setting the link between policy and the economy is much looser than today: the fledgling Republic of China lacked a central bank regulating money supply and credit, and private ones issued money and loans.[5] Third, the main radical movement in 1930s China, the Communist Party, had a well-defined social target in the urban areas: the working class, and in particular factory workers.[6]

We provide micro-econometric evidence of the shock's impact on social unrest, based on novel, hand-collected archival information on credit, labor relations, and fringe political activity in 1930s China. Our data reconstruct a Chinese "credit registry" for the period 1931-1935, and document firm-level labor unrest episodes in three major Chinese cities (Nanjing, Shanghai, and Tianjin), as well as Communist Party penetration among workers at firms located in Shanghai.

We exploit cross-sectional variation in the exposure of lenders and borrowers – banks and firms – to the Silver Purchase shock to identify the direct effect of credit rationing on social unrest, measured by labor unrest intensity and Communist Party penetration. Our empirical strategy is articulated in two steps. First, we test if there is a lending contraction, and banks with a larger exposure to the Silver Purchase shock (lower pre-1933 silver reserves) curb credit after 1933. Second, we study whether labor unrest episodes and the spread of Communism in Chinese firms

[4] For instance, delimiting the social support for right-wing parties in Europe is notoriously challenging (Arzheimer (2009)), and the voter base of modern fringe movements transcends traditional boundaries between the political right and left (Cramer Walsh (2012), Jacoby (2014)). More recent work shows how the support to populist movements is associated with a number of voter characteristics (Becker, Fetzer, and Novy (2016), Guiso et al. (2017)).

[5] Government budgets at the time were limited, and fiscal policy was not generally considered a tool to mitigate economic shocks, especially in an emerging economy such as the Republic of China.

[6] In a speech given on June 30, 1949, to commemorate the 28th anniversary of the Chinese Communist Party, Chairman Mao Zedong declared: "The people's democratic dictatorship needs the leadership of the working class. (…) The entire history of revolution proves that without the leadership of the working class revolution fails and that with the leadership of the working class revolution triumphs."

relate to their banks' silver reserves. If banks exposed to the Silver Purchase shock cut lending, firms borrowing from them face tighter financial constraints, which limit investment and lead to pay cuts and layoffs, increasing the likelihood of labor unrest and Communist support. Our evidence supports these arguments.

While throughout the analysis we pay great attention to identification and what we can and cannot conclude in causal terms, our main results are immediately visible in the data displayed in Figures 1 and 2. Chinese credit sharply contracts over 1933-35: credit-to-GDP drops by about 15% (Fig. 1.A), and credit-to-deposits by 10% (Fig. 1.B). As we show in Figure 2, this is driven by banks with lower pre-1933 silver reserves (panel A). Firms borrowing from these banks, in turn, experience increased labor unrest intensity and Communist party penetration. By 1935, labor unrest episodes (panel B) and Communist penetration (panel C) are about twice more likely than at firms borrowing from banks with larger reserves.

Our tests exploit the wealth of micro-level information in our data to interpret these facts in a causal sense. First, we show that banks with lower pre-1933 silver reserves reduce lending volumes after 1933. Because we are able to observe bank-firm lending relationships, we can absorb credit demand with borrowing-firm fixed effects: the *same* firm, borrowing from multiple banks, experiences restricted lending from those banks that are more exposed to the Silver Purchase. Therefore, the shock appears to ration credit.

Second, we look at social unrest. As a first gauge, we document that firms that are more exposed to the Silver Purchase shock experience a disproportionate increase in labor unrest after 1933. We measure a firm's exposure by its access to silver reserves pools, either from banks with which it has an ongoing lending relationship, or associated with banks with branches near the firm's location. Smaller reserves pools are related to a larger increase in labor unrest intensity. Our estimates imply that borrowers with access to the smallest reserves pools experience a 30% larger increase in the number of labor unrest episodes, and a 15% longer average episode duration, in comparison to firms borrowing from banks with the largest reserve pools.

As a second measure of social unrest, we study fringe political activity, proxied by Communist Party penetration. Our regressions show that firms with access to the smallest reserves pools experience a 3 to 6.5% larger increase in communist penetration, relative to firms with access to the largest reserve pools. Limited access to credit resulting from the Silver Purchase, thus, has

important socio-political consequences, exacerbating labor relations and, to a more modest extent, affecting the reach of the Communist Party.

Additional tests rule out alternative interpretations of our findings. First, the effects we uncover are unlikely driven by omitted factors affecting East Asian economies in the 1930s. Comparing industries exposed to versus isolated from international trade, to assess the impact of a mechanical currency appreciation driven by the Silver Purchase, we find statistically indistinguishable effects across the two groups. This indicates that an exchange rate channel is unlikely behind our results, and alleviates concerns about a worldwide trend towards greater instability associated with the 1930s Great Depression: international trade exposure does not appear to mediate our effects. In addition, there is no evidence of similar effects in Hong Kong, the closest economy to 1930s China. Finally, our results are robust to excluding firms related to Japanese interests, suggesting that they are not driven by Japanese political interference.

Second, we address the potential endogenous selection of banks into high and low pre-1933 silver reserves groups. We exploit a unique feature of 1930s China's monetary system: the parallel circulation of a traditional currency backed by copper, whose availability is exogenously determined by the geographical distribution of copper mines. Copper coins circulated locally and were typically used to clear small transactions, as a partial substitute to Chinese silver dollars, in regions with a relative abundance of copper. We use copper availability at local mines as an instrument for the demand for silver-backed currency, and thus pre-1933 reserves. Instrumental variables estimation confirms our results, indicating that they are not driven by selection.

Third, for a sample of textile mills for which the information is available, we find that firms with access to a smaller silver reserves pool are more likely to reduce employment, electricity consumption, and output after 1933. This result indicates that labor unrest episodes (and, potentially, Communist penetration) are indeed related to worsening economic conditions driven by the Silver Purchase shock. It is also consistent with labor unrest statistics, showing that a large majority of unrest episodes are due to salary cuts and employees layoffs.

Our results speak to a growing literature relating economic shocks to social and political outcomes (de Bromhead, Eichengreen, and O'Rourke (2013); Dube and Vargas (2013); Bai and Jia (2016); Funke, Schularick, and Trebesch (2016)). The literature has not yet reached a consensus on whether economic shocks have a socio-political effect. Some studies argue, for instance, that austerity does not have an impact on political stability (Alesina et al. (1998); Alesina, Carloni, and

Lecce (2011)), others find that it leads to a rise in social and political unrest (Ponticelli and Voth (2017)). Focusing on the Silver Purchase episode, and using micro data, helps us identify a causal channel running from a negative economic shock – the 1930s Chinese credit crunch – to social unrest. While we study a credit shock, our findings can be generalized to any shock affecting firms' investment prospects. We also contribute to the literature on the economic determinants of labor unrest (Kennan (1986); Naidu and Yuchtman (2015)). In this literature too, a general challenge is that the likelihood of labor unrest and firm behavior are jointly determined. Our empirical framework allows us to examine how an exogenous shock to the firm's access to credit affects labor unrest propensity.

Our work also provides new evidence on the effects of the Silver Purchase Program on the Chinese economy. This is considered a key moment in Chinese economic history, and throughout the years it has received the attention of many prominent scholars (Friedman and Schwartz (1963), Brandt and Sargent (1989), Rawski (1989, 1993), Friedman (1992), Burdekin (2008)). We contribute to the study of this historical episode by bringing to the table new micro-level data that allow us to focus on a specific channel, through which the Program may have affected social unrest and the real economy: credit.

Finally, we contribute to the literature on the real effects of bank liquidity shocks. This literature has focused on the identification of credit supply effects, e.g. via natural experiments (Khwaja and Mian (2008); Schnabl (2012); Chodorow-Reich (2014); Cingano, Manaresi, and Sette (2016)). Our setting combines a plausibly exogenous shock – the U.S. Silver Purchase – with micro-level data on labor unrest and political extremism, providing evidence on so far unexplored real effects of credit shocks, and suggesting that restrictions to finance and credit can be a powerful trigger of social unrest.

The remainder of the paper is organized as follows. Section II provides the historical background. Section III presents the data. Section IV presents our tests. Section V discusses the interpretation of our results. Section VI concludes.

## 1.2. Historical background

*A. Credit and silver in 1930s China*

In the early 1930s, Chinese banks are divided into two categories, "modern" and "native." The *Chinese Banker's Yearbook* (全国银行年鉴) reports 176 "modern" domestic banks in China, with over 1,300 branches (see also Liu (2007)). These banks can issue currency (e.g. to make loans), subject to a reserve requirement: the bank must hold silver reserves corresponding to at least 60% of the nominal amount of banknotes it issues (the remaining 40% consisting of government bonds). In order to make a new loan, thus, the bank can draw on its reserves in excess of the 60% threshold, or purchase silver on the market to back the lending amount exceeding its reserves. Silver reserve ratios range from 60% to 100% and are around 66% on average, so that different banks have a different exposure to the Silver Purchase shock (also see next section).

Silver reserves are reported on the assets side of bank balance sheets, at the official parity established by the Treasury. This implies that an increase in the market price of silver does not directly increase the asset value of the bank. A bank can only capitalize the increase in silver prices by redeeming banknotes, obtaining silver and recording in the balance sheet the corresponding amount at market prices, effectively, reducing money supply.

The four largest modern banks – Central Bank of China, Bank of China, Bank of Communications, and Farmers Bank of China – have a closer relationship with political power, and perform duties such as placing Treasury bonds on the market (Tamagna (1942, p. 121)). There is, however, no central bank in the modern sense, entrusted to set interest rates or to regulate the money supply.

The "native" banks are smaller, operate locally, and often lack limited liability (Tamagna (1942, p. 57-59)). They mainly circulate banknotes issued by the modern banks. In addition, they may issue in their own name banknotes backed by copper, for local circulation (Tamagna (1942, p. 68)). Although our data do not include native banks (to the best of our knowledge, no records of their balance sheets and loans survive), we exploit their issuance of copper-based currency in a robustness check in Section V.

*B. The Silver Purchase program*

The Silver Purchase program is initiated in May 1933, establishing that the U.S. government can monetize silver (in addition to gold) to back a money supply expansion. The Roosevelt administration orders U.S. mints to buy all newly produced U.S. silver at 64.64 cents per ounce, at a time when the market price is 44 cents (Friedman and Schwartz (1963, p. 483)). The world

price of silver nearly doubles in the space of two years, reaching about 70 cents per ounce in New York in 1935 (Figure 3.A).[7]

The Roosevelt administration undertakes the Silver Purchase program to accommodate lobbying in the senate by the so-called "silver bloc."[8] Between 1928 and 1932, the price of silver has dropped by 30%, and silver producers increasingly demand Federal intervention to reverse this trend. Out of 14 silver-bloc Senators, 12 are Democrats like Roosevelt, and strongly advocate policies to raise silver prices. Their interests are also backed by states with large agricultural sectors, which aim to increase inflation and raise agricultural prices. In 1934, the Silver Purchase Act further empowers the Federal Government to purchase silver at home and abroad.

Rising silver prices have a visible impact on the quantity of silver available in China, as large amounts are exported to take advantage of the high market price. The Chinese silver stock growth rate takes a sharp downward turn after 1933, reducing the stock by about 15% by 1935 (Figure 3.B).[9] Foreign banks operating in China, such as HSBC and the National City Bank of New York, appear to be the main drivers of the silver export, along with wealthy individuals opening accounts abroad in foreign currencies (Tamagna (1942, p. 104), Cheng (1956, pp. 260-261), Shiroyama (2008, p. 157)).[10] These trends have the potential to impair Chinese banks' lending capacity. China does not have silver mines and does not produce silver; therefore, in order to issue currency to make loans, domestic banks typically purchase silver from foreign ones. This becomes more expensive, as silver prices rise.

Figures 1.A and 1.B show the time series of credit in China between 1931 and 1935. Figure 1.A plots aggregate credit over GDP. While the ratio increases by about 3% between 1931 and 1933, it sharply declines by about 15% between 1933 and 1935. Figure 3.B illustrates loans-to-assets and loans-to-deposit ratios, which also sharply decline after 1933.

[7] The London price of silver registers a similar rise as on the New York Market over this period.

[8] The "silver bloc" states are: Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah (Kreps (1934, p. 246), cited by Friedman (1992)).

[9] After 1933, the silver holdings of foreign banks based in Shanghai drop by about one half, corresponding to over 20% of the total silver holdings of banks based in Shanghai (Appendix Figure A.2).

[10] To the best of our knowledge, there is no evidence that Chinese banks actively exported silver to realize capital gains. In fact, it appears that they were concerned with the situation, and tried to reach an agreement with foreign banks to stop the silver outflow (Tamagna (1942, p. 103)). For example, the 1934 annual report of the Bank of China declares that "the silver problem concerns both domestic and foreign business. We eventually share the consequences together. […] Chinese banks feel the responsibility to maintain economic stability, and request foreign banks to work together […]. Otherwise, industries will suffer from credit reduction."

Unable to stem the effects of the Silver Purchase shock, the Chinese government finally abandons the silver standard in late 1935. An official announcement is made in November, declaring all silver to be government property. All silver exchange is forbidden, and paper banknotes are issued one-to-one against the silver Chinese dollars in circulation.[11]

Our analysis explores credit as a channel through which the silver outflow may have an impact on social unrest. While the economic historiography debates whether the silver purchase program materially reduced the Chinese money supply (see Rawski (1989, 1993) and Brandt and Sargent (1989), as well as Appendix A.1), our mechanism relies exclusively on the increase in silver prices and decline of silver stock documented in Figures 3.A and 3.B. We present a model describing it in greater detail in Appendix A.2.

*C. Limited impact of confounding events in 1931-1935*

In 1931-1935 China enjoys a relatively stable government and internal politics (Cheng (2003, p. 67)). Moreover, we find that the only major external event, the 1931 Japanese invasion of Manchuria, does not appear to have a tangible impact on credit provision in the main urban areas covered by our sample data, which are located far from it.

In 1928 the Nationalist government led by Chiang Kai-shek reunifies the country after a decade of civil war, bringing along a period of relative stability that allows the economy and the banking sector to grow (Cheng (2003, pp. 67-70)). There are still skirmishes in rural areas with various guerrilla groups, but the Nationalists effectively control most of the country, especially the urban areas where economic activity concentrates. In particular, the data in our sample focus on cities under Nationalist control, free of guerrilla episodes in 1931-35.

Ho and Li (2013) document that the only major political event in this period is the Japanese invasion of Manchuria, which begins on 18 September 1931.[12] The invasion raises concerns about the solvency of the Chinese government, leading to a partial restructuring of Treasuries in February 1932.

[11] Before 1935, the Chinese government imposes high export duties on silver, with the aim of curbing profits on silver exports. Official Chinese customs data show that the silver outflow is close to zero during 1935. However, smuggling makes this regulation ineffective: estimated silver smuggling amounts between 1934 and 1936 are roughly 250 million Chinese silver dollars. Towards the end of 1935, at the end of our sample period, the Chinese government becomes the controlling shareholder of two "modern" banks, the Bank of China and the Bank of Communication, in an attempt to boost the credit capacity of the two institutions (Cheng (2003, p. 99)).

[12] The only other two major events in the 1921-42 period identified by Ho and Li (2013) are the 1927 Northern Expedition and the 1937 Sino-Japanese war; both take place well outside our sample period.

This event alone appears unlikely to have a major impact on our tests, for four reasons. First, it happens at the beginning of our sample period, with little detectable impact on credit provision. In fact, the aggregate credit-to-GDP ratio slightly increases between 1931 and 1933 (Cheng (2003) and Liu (1946), as well as Figure 1). Second, the restructuring involves a reduction of coupon rates and an extension of maturities, while face values remain unchanged (Cheng (2003, p. 124)). Treasuries can form up to 40% of bank reserves, on the basis of their *face value* (see Appendix A.2). Thus, the restructuring requires no adjustment to the outstanding amount of currency. Third, the time series of Chinese sovereign debt yields does not exhibit a strong reaction to the event. In fact, the spread relative to British Gilts slightly *drops* towards the end of 1931 (Goetzmann, Ukhov, and Zhu (2007); Ho and Li (2013)). Fourth, Manchuria itself has very limited relevance for the Chinese banking industry in the early 1930s: no modern banks are headquartered there, no loans in our sample are made to firms operating in Manchuria, and although several banks have branches in Manchuria, those branches account for only about 2% of the total number of bank branches in the country.[13]

One last potential challenge is that banks may cut loans collateralized with Treasury bonds. Our data, however, reveal that less than 2% of outstanding loans have Treasury bonds as collateral in the first place. We also find a very low correlation (0.05) between banks' silver reserves and loan collateralization with Treasuries.

In sum, other events taking place over 1931-1935 have only a modest effect on credit provision. The Silver Purchase shock is the major event with the potential to affect credit during our sample period.

## 1.3. Data

We build our analysis on four main sources providing information on: (1) Loan contracts; (2) Bank balance sheets; (3) Labor unrest episodes; and (4) Underground Communist Party activities at our sample firms. All of our data refer to the years starting in 1931 and ending in November 1935, when the Republic of China abandons the silver standard.

*A. Loan contracts*

[13] Data on bank and bank branches' location are retrieved from the *Chinese Banker's Yearbook* (全国银行年鉴).

Individual loan information is collected from provincial and city archives in seven major Chinese provinces/cities: Beijing, Chongqing, Guangzhou, Nanjing, Shandong, Shanghai, and Tianjin. These areas are chosen because of their economic importance in inter-war China: Beijing is the former imperial capital, with considerable industrial activities; Chongqing and Guangzhou are among the oldest and largest trading harbors; Nanjing is the capital city; Shandong is a major industrial and farming province in North China with a large population; Shanghai and Tianjin are the main financial centers. Individual loan contracts report the issuing bank's name, the identity of the borrowing firm, the loan amount, issue date, and for a subset of the contracts also additional terms such as interest rate, duration, collateral, or the purpose of the loan. The loan amount is the most widely populated data item, so we focus on it for our tests.

In total, the sample covers 579 industrial loans, made by 32 financial institutions to 151 individual plants, associated with 139 firms. The mean (median) loan in our sample amounts to 273,000 (44,000) Chinese dollars.[14] The lenders in this set appear to be representative of the domestic banking sector in 1930s China, and comprise three large banks (Bank of China, Central Bank of China, and Bank of Communications), 27 other modern banks, and two other financial institutions (Shanghai Trust Co., Ltd.; and Joint Savings Society of Yienyieh, Kincheng, Continental and China & South Sea Bank).[15]

Based on the available information from the loan contracts, our sample borrowers are also representative of the 1930s Chinese economy. They span 17 different industries, out of a total of 27 industries based on the International Labor Organization 1923 classification in use in 1930s China.[16] The most important industries in our sample are transportation, services, and textiles (25%, 22%, and 12% of the aggregate loan volume, respectively), consistent with the massive

[14] As a benchmark, Zhang (2011) reports an average hourly wage for a male worker in Shanghai in 1931-33 of about Ch$0.10, and a 70-hour working week, implying a yearly wage of Ch$364.

[15] We are able to recover 792 individual loan contracts, out of which 579 can be matched to banks covered by our data. Among these loans, we exclude 52: 47 loans to non-profit institutions (universities, colleges, and high schools), four loans received by the *Hunan Flood Committee* (湖南水灾善后委员会, a charity), and one loan contract with unreadable identifying information. All the results are robust to including these 52 loan contracts. The distribution of lenders in this sample reflects the overall Chinese banking industry (based on the data described below, in section III.B). The rank correlation between the banks' shares of loans in this sample and their overall credit market share is 73% (p-value < 0.01), and a Pearson chi-square test cannot reject the null hypothesis that the loan sample and overall credit market share distributions are identical.

[16] We add a residual category "Other" for one contract, in which the borrowing firm operates across multiple industries (Power and utilities, Services, and Construction). In addition, there are 103 contracts with no available industry classification, which we still use in our analysis.

railway construction underway during the period (Ma and Zhang (2007)), as well as the historical role of textiles in Chinese industrial development (Young (1971, p. 306)).

*B. Bank balance sheet data*

Bank balance sheet data are retrieved from the *Chinese Banker's Yearbook* (全国银行年鉴), published by the Bank of China, and the *Bankers' Weekly* (银行周报), a review published by the Shanghai Banking Association on a weekly basis from May 1917 through to March 1950. Each issue contains the annual reports of both national and regional banks, as well as the leading trusts.[17]

We complement these data with information from two additional sources: the *Financial and Commercial Monthly Bulletin of the Bank of China* (FCMB, 中外商业金融汇报) issued by the Bank of China from 1934 to 1939, and Liu (2007). The FCMB is a widely adopted, reliable source providing data on the Chinese banking sector during the first half of the 20th century. It reports data on banks' banknote issuance and the related silver stock.[18] Liu (2007) reports complementary information on bank location and capital.

From these sources, we retrieve data on bank total assets, equity, cash holdings, outstanding loans, deposits, net income, retained earnings, banknotes issuance, and silver reserves. The key variables of interest in our analysis are $Silver$, the (log-)stock of silver held by the bank, $Excess\ silver$, defined as the natural logarithm of the difference between the bank's silver stock and the 60% silver reserve requirement, and $Excess\ reserves$ $(Y/N)$, an indicator variable equal to 1 if the bank's silver stock is (strictly) greater than the 60% requirement. We obtain information for 138 institutions (126 banks and 12 other financial institutions). Out of these 138, we have complete balance sheet information for 80 banks, which account for about 95% of Chinese modern banks' assets over our sample period (Cheng (2003, Appendix II)).

We present descriptive statistics in Table 1. Prior to the implementation of the Silver Purchase program, there is significant cross-sectional dispersion in the level of silver reserves for our sample banks. The average bank has silver reserves of 1.9 million Chinese dollars in 1931. The minimum silver reserve we observe is exactly zero dollars, for the 51 banks in our sample that

[17] In 1930s China, trusts engage in financial intermediation activities, including collecting deposits, extending loans, and selling insurance. They do not materially differ from banks in terms of savings and lending practices, so we include them in our data (all the findings are robust to excluding them).

[18] The *Financial and Commercial Monthly Bulletin of the Bank of China* (中外商业金融汇报) is issued by the research department of the Bank of China (中国银行总管理处经济研究室).

do not issue any banknotes, while the maximum value is 64.4 million dollars.[19] Around 62% of those banks that issue banknotes hold exactly the 60% minimum silver reserve; the remaining banks hold excess reserves, ranging between 60-100%, with an average level of 66% and a standard deviation of about 9% of the total currency issued.

*C. Labor unrest*

Information on labor unrest episodes in major Chinese cities around the Silver Purchase program is retrieved from surveys on labor relations set up by the Republic of China's central government and local authorities. These records provide information about labor unrest episodes revolving around disagreements between employers and employees, which in a number of cases involve acts of violence.

These data are available for three major cities: Nanjing, Shanghai, and Tianjin. We retrieve the Nanjing data from the surveys *Industrial Disputes in Nanjing 1932-1934* (南京市之劳资纠纷统计) and *Industrial Disputes in Nanjing 1935* (民国二十四年南京市劳资纠纷统计), which record cases that are reported and processed by the Bureau of Social Affairs of the city of Nanjing. Information about Shanghai is retrieved from the survey *Industrial Disputes in Shanghai since 1928* (近五年来上海之劳资纠纷), conducted by government of greater Shanghai between 1931 and 1935.[20] We complement these data with information from the surveys *Strikes and Lockouts in Shanghai since 1918* (近十五年来上海之罢工停业) for the period 1931-1932, and *Strikes and Lockouts in Shanghai in the Past Four Years* (近四年来上海的罢工停业) for 1933-1935. Regarding Tianjin, we use information available in the *International Labor Bulletin* (国际劳工通讯; 1934-1941). These data are complemented with additional information retrieved from two newspapers, the *Yishi Bao* (益世报; a Tianjin daily) and the *Shen Bao* (申报; a Shanghai newspaper covering stories from other parts of China).

We identify in total 1,209 episodes of labor unrest between 1931 and 1935 (Table 2.A). For episodes taking place in Shanghai, the data report information on the underlying reason. As illustrated in Figure 4, the majority are related to worsening economic conditions: the top causes are layoffs (56%) and salary disputes (21%).

[19] We retain in the sample banks that do not issue banknotes, as they constitute a useful control group. Results remain unaltered if exclude them from the sample.

[20] For the years between 1933 and 1935, we retrieve the survey data from the *International Labor Bulletin* (国际劳工通讯; 1934-1941, No. 5 Issue 6, June 1937), compiled by the Chinese Branch of the International Labor Organization.

*D. Communist activities*

The final piece of data is about underground Communist Party activities at Shanghai factory plants during our sample period. We obtain these data from the Shanghai Municipal Police Files, 1894-1949 (henceforth, SMP files). The SMP files contain the records of the British-run municipal police force in Shanghai, which investigates and reports on subversive activities in the city, including communist ones.

The SMP files do not have a standardized format, as they are mostly internal reports documenting the work of this special police force. We focus on two types of documents: arrests of communist supporters and intelligence reports. The arrests provide information about individuals who are taken into custody by the Shanghai Municipal Police, and indicate the name of the firm where the arrested individual works. The intelligence reports describe the activities of undercover agents who infiltrate communist cells in Shanghai. They provide detailed accounts of the cells' meetings, including lists of firms or factories that a given cell targets for recruitment into the party, as well as information about the workplace of participants to the meeting. They also investigate a number of strikes and labor unrest episodes, for a connection to communist activities.

This archival work results in a list of Shanghai firms penetrated or targeted by communists during our sample period. From these records, we find that about 96 plants, belonging to 65 firms (corresponding to 159 plant-year observations) have communist sympathizers among their workers, or have been targeted by the communists for recruitment.

## 1.4. Empirical analysis and results

We use our data to test the impact of the credit shock on social unrest. Our empirical strategy is articulated in two parts. First, we test if the U.S. Silver Purchase program leads to a contraction of lending in China. Second, we test if there are social consequences to the credit shock, in terms of labor unrest and Communist Party penetration.

*A. Impact of the Silver Purchase program on lending*

In order to make a loan, a bank needs to draw on its silver reserves in excess of the 60% requirement, or acquire additional silver on the market. A higher market price of silver, thus, increases the cost of lending for the bank, particularly for banks with ex ante lower silver reserves.

As a result, banks that are more exposed to the Silver Purchase shock, i.e. with lower pre-1933 silver reserves, will drive the post-1933 credit contraction.

We use three alternative measures of silver reserves: the bank's 1931 (log-)silver holdings ($Silver$), the difference between the bank's silver holdings and 60% of their outstanding banknotes in 1931 ($Excess\ silver$), and an indicator variable equal to 1 if the bank's silver stock is greater than the 60% requirement in 1931 ($Excess\ reserves\ (Y/N)$).[21] These measures are consistent with the predictions of a simple model we present in Appendix A.2, which rationalizes bank lending in 1930s China. The model predicts that the amount of loans issued by a bank depends on the bank's silver holdings and on whether they exceed the 60% silver reserve requirement.

We first regress the (log-)loan volume from the banks' balance sheets on these variables, estimating:[22]

$$L_{bt} = \alpha + \beta Silver_{b,1931} + \gamma Post_t \times Silver_{b,1931} + \delta Post_t + \mu' x_{bt} + \varepsilon_{bt} \qquad (1)$$

The dependent variable is the natural logarithm of the dollar amount of loans made by bank $b$ in year $t$. We regress this variable on an indicator $Post$, equal to 1 in the years subsequent to the U.S. Silver Purchase program (1933 onwards), the bank's 1931 silver reserves $Silver$ (or $Excess\ silver$), and an interaction term, as well as a vector $x$ of control variables. A positive $\gamma$ coefficient in equation (1) indicates that banks with larger silver reserves before the shock extend larger loans after 1933. We estimate (1) by collapsing the data down to bank averages before and after 1933, to be immune to the Bertrand, Duflo, and Mullainathan (2004) critique, and first differencing, i.e. we run:[23]

$$\Delta L_b = \delta + \gamma Silver_{b,1931} + \mu' \Delta x_b + \varepsilon_b \qquad (1')$$

where $\Delta$ denotes first differences, so that $\Delta L_b$ is the change in average log-loans from before to after 1933 for bank $b$.

The estimates, reported in Table 3, are consistent with the evidence from Figure 2, and with the notion that the Silver Purchase Program leads to a credit contraction: banks with lower reserves reduce their lending volumes after 1933. The result holds across all three proxies for a

[21] Whenever the 1931 silver reserves value is not available, we use in its stead the 1932 value. We use the earliest available silver reserves to guarantee that they are pre-determined relative to the Silver Purchase shock of 1933; analogous results obtain if we use the 1931-1933 average reserves instead (available upon request).

[22] In these tests, we exclude the amount of loans extended to the government from each bank's total loan volume. Our results hold, quantitatively and statistically, if we do not exclude loans to the government.

[23] Equivalently, we may not collapse the data and estimate a panel regression with fixed effects, clustering the standard errors. This approach delivers similar results, reported in Appendix C.

given bank's exposure to the shock ($Silver$, $Excess\ silver$, and $Excess\ reserves\ (Y/N)$), but the economic magnitudes are best assessed by looking at $Excess\ reserves\ (Y/N)$. The coefficient estimate of 0.13 (specifications (6)-(7)) implies that, in comparison to banks that are less exposed to the shock (i.e., having 1931 silver reserves in excess of the 60% threshold), banks that are immediately exposed to it reduce their loans by 13% more per year on average during the period 1934-1935.

These results suggest a severe impact of the shock on credit provision. The estimates are based on a sample representing over 95% of the total assets of modern banks active throughout the sample period, and are thus largely free from selection or survivorship bias. They could, however, be confounded by credit demand effects associated with individual firms. For instance, banks with larger silver reserves might tend to lend to more efficient firms, or less risky firms with a lower exposure to the international economic crisis of the 1930s. This would also predict higher lending growth for banks with larger silver reserves – but due to credit demand, not supply.

To address this identification challenge, we turn to our data on matched bank-firm individual loan contracts. Following the literature on bank liquidity shocks (Khwaja and Mian (2008); Schnabl (2012)), we absorb the impact of credit demand by controlling for firm fixed effects, interacted with time, in the following specification:

$$L_{fbt} = \alpha_{f0} + \alpha_f \times Post_t + \beta Silver_{b,1931} + \gamma Post_t \times Silver_{b,1931} + \mu' x_{fbt} + \varepsilon_{fbt} \qquad (2)$$

where the dependent variable is the natural logarithm of the dollar amount lent to firm $f$ by bank $b$ in year $t$. Again, a positive $\gamma$ coefficient indicates that banks with larger silver reserves before the shock make larger loans after 1933; as before, we estimate (2) by collapsing the data down to firm-bank pair averages before and after 1933.

Identification in equation (2) mostly originates from the cross-sectional differences in our sample banks' 1931 silver reserves.[24] Banks with a larger amount of pre-shock silver reserves are better able to absorb the liquidity shock, and are thus less likely to ration credit after 1933. As Khwaja and Mian (2008), we then restrict the sample to the set of firms that borrow from at least two banks, allowing us to control for firm fixed effects.

[24] In results omitted for brevity, we find that the industry distribution is statistically very close for "treated" and "control" firms in this sample (i.e. firms whose lenders have silver reserves above/below the median). The rank correlation of industry shares across the two groups is 61% (p-value < 0.01), and a Pearson chi-square test cannot reject the hypothesis that the distributions of industry shares in the two groups are identical.

We report the estimates of (2) in Table 4. They are consistent with our earlier results: banks with a larger exposure to the Silver Purchase program (lower pre-1933 silver reserves) are quicker to cut down lending. The point estimates are remarkably stable across specifications with and without firm fixed effects. Focusing again on the coefficient on $Excess\ reserves\ (Y/N)$, they imply that banks immediately exposed to the Silver Purchase shock curb lending by about 30% more than banks with excess silver reserves. Moreover, our empirical strategy alleviates the potential confounding effect of loan demand by individual firms. The presence of borrowing firm fixed effects in the regression equation implies that the *same* firm, borrowing from two different banks, will experience a larger drop in lending from the bank with lower silver reserves, i.e. greater exposure to the shock.

Taken together, these findings provide the first block of evidence for our analysis. Importantly, we obtain very similar results from bank-level and loan-level regressions. The former does not suffer from selection bias, but the estimates may be contaminated by an effect of credit demand. The latter look at a selection of loan contracts, but the detail of the data allows us to make stronger statements in terms of causality. The scarcity of silver in China, and its high market price, driven by the U.S. Silver Purchase program, leads to a reduction in credit, the more severe the lower pre-1933 silver reserves. As we will also show in section V, the credit contraction is unlikely to be explained by demand conditions, supporting a causal interpretation for our evidence.

*B. Impact on social unrest*

Next, we look at the consequences of the Silver Purchase program shock on social unrest, focusing on labor unrest episodes and Communist Party penetration.

In the first place, we find that labor unrest is closely related to firm-level economic conditions. This supports the argument that the credit contraction can be expected to have an impact on labor unrest, as opposed to alternatives such as a fight for civil rights or political freedom. As illustrated in Figure 4, the majority of labor unrest episodes in our data are due to layoffs or salary disputes. Only rarely we find labor unrest motivated by other causes. In line with these arguments, for a subsample of cotton mills where more detailed information is available, we find that worsening economic conditions are related to the Silver Purchase shock. In particular, firms borrowing from low-silver reserves banks are more likely to lay off workers, as discussed in section V.D.

It also appears that the loans in our sample are evenly distributed between financing investments projects and working capital and/or wage payments (Figure 5). In both cases, a credit supply cut may have an impact on the labor force either because planned investment projects need to be scrapped or because the firm lacks the resources to maintain production.

In our main social unrest tests, we rely on the fact that 1930s Chinese firms borrow primarily from banks headquartered near them, or with branches in their proximity, as argued by the literature on relationship lending (Petersen and Rajan (2002); Degryse and Ongena (2005)) and verified in our data (see below). Building on this fact, we develop an index of local silver reserves availability around each firm $f$ in our sample, as an inverse distance-weighted average of bank silver reserves:

$$Silver\ pool_f = \sum_b \frac{Silver_b/d(f,b)}{\sum_b 1/d(f,b)} \quad (3)$$

where $Silver_b$ denotes the log-1931 silver reserves of bank $b$, and $d(f,b)$ is the distance between firm $f$ and bank $b$ (or its branches), measured in km.

$Silver\ pool$ is larger if banks in the vicinity of firm $f$ have larger silver reserves. Similarly, we define $Excess\ silver\ pool$, a weighted average $Excess\ silver$, and $Excess\ reserves\ (Y/N)\ pool$, a weighted average $Excess\ reserves\ (Y/N)$. Importantly, these measures capture the capacity of the pool of *potential* lenders of firm $f$ to absorb the Silver Purchase shock (similar results obtain if we focus on its actual lenders, as we discuss below).

We then relate the index to the measures of social unrest intensity. We estimate:

$$Unrest_{ft} = \alpha + \beta Silver\ pool_f + \gamma Post_t \times Silver\ pool_f + \delta Post_t + \mu' x_{ft} + \varepsilon_{ft} \quad (4)$$

where, depending on the specification, $Unrest$ indicates the (log-)number of labor unrest episodes at firm *f* in year *t*, the duration of these episodes, or (log-)number of times firm *f* is mentioned in the SMP files as experiencing communist activities in year *t*. As before, we collapse the sample to plant averages before and after 1933 following Bertrand, Duflo, and Mullainathan (2004) and estimate (4) on changes. The control variables $x$ include city-district, industry, and firm nationality fixed effects.[25]

[25] Firms with access to large/small (above/below the median) silver reserve pools are ex ante similar. In particular, in results omitted for brevity we find that the rank correlation of industry shares in the two groups of firms is 90% (p-value < 0.01), and a Pearson chi-square test cannot reject the hypothesis that the industry shares distributions are identical.

We examine labor unrest episodes in Table 5. Specifications (1)-(3) of panel A focus on the (log-)number of labor unrest episodes in a given year, and specifications (4)-(6) on their duration. Across all specifications, $Silver\ pool$, $Excess\ silver\ pool$, and $Excess\ reserves\ (Y/N)\ pool$ are negatively associated with the number of labor unrest episodes and their duration. This is consistent with the notion that firms that have access to a smaller pool of silver reserves are more exposed to the credit shock, and experience intensified labor unrest. The effect is also important in economic terms: firms with the lowest $Excess\ reserves\ (Y/N)\ pool$ experience a 30% higher increase in the number of labor unrest episodes after 1933 than firms with the highest $Excess\ reserves\ (Y/N)\ pool$; the corresponding difference in terms of unrest episode duration is 15%.[26] Importantly, these results control for city district fixed effects; that helps us to rule out the possibility that local economic conditions (for instance, real estate prices) may drive our results.[27]

Panel B of Table 5 reports two sets of tests. First, in column (1) we validate the conjecture that firms tend to borrow from the banks that are geographically closer to them. We rely on information on existing lending relationships from our loan contracts data, and regress an indicator variable equal to 1 if a given firm borrows from a given bank on the natural logarithm of the distance between the firm and the bank (in km), and indicators for the firm's location (city).[28] Corroborating our approach in panel A, we find a strong negative relation between distance and the likelihood of a lending relationship: a 10% closer bank is 0.18% more likely to have a lending relationship. In our data, a firm has a lending relationship, on average, with 4% of the banks with a branch located in its city, implying that the estimated effect of distance is material.

Second, we validate our tests of panel A by looking at actual lending relationships. In this case, the sample size shrinks, because we are restricted to working with firms and banks where information is available from our loans data. Despite the sample shrinkage, in columns (2)-(4) we

[26] These effects are estimated as follows. The minimum value of $Excess\ reserve\ (Y/N)\ pool$ is 0.38, and the maximum 0.82 (Table 2.A). Based on the coefficient estimate of –0.675 from Table 5.A (specification (3)), this implies a $-0.675 \times (0.38 - 0.82) = 30\%$ higher increase in the number of labor unrest episodes for firms at the lowest level of $Excess\ reserve\ (Y/N)\ pool$, relative to firms at the highest level. The effect on the duration of unrest episodes is estimated similarly.

[27] Because we run the test on collapsed data, effectively we are controlling for district fixed effects interacted with the $Post$ indicator.

[28] The unit of observation in column (1) is a bank branch-firm pair. For each bank, we include in the sample the branch that is closest to the firm. The dependent variable takes the value of 1 if a given branch belongs to a bank that has a lending relationship with the firm, and 0 otherwise.

are able to detect a significant relation between silver reserves and lending outcomes, in line with the results described in panel A. Across all silver holdings measures, we find a negative relationship between silver reserves and the post-1933 number of labor unrest episodes.[29] Firms borrowing from banks without excess reserves experience an 18% higher increase in the number of labor unrest episodes after 1933 than firms with access to excess reserves, economically close to the effects implied by the estimates of panel A.

We then turn to communist penetration as a measure of social unrest. Table 6 relates silver reserves to the probability that a firm experience underground Communist Party activities. Although positive as could be expected, at 13.5% the correlation between the frequency of labor unrest episodes and communist activities is not high, suggesting that they capture different facets of social unrest. We find a negative relationship between communist activities and access to silver reserves. Whether communist activities propagate because workers at firms with little access to silver reserves spontaneously radicalize, or because the Communist Party targets exposed firms to recruit their workers, this result suggests a causal channel from credit provision to the spread of social unrest.

The estimates imply that firms with access to the smallest pool of excess reserves (i.e. with the lowest $Excess\ reserves\ (Y/N)\ pool$) experience a 3 to 6.5% larger increase in communist penetration after 1933. In terms of statistical significance, the results are weaker compared to the labor unrest results and, in particular, they are not significant for $Excess\ reserves\ (Y/N)\ pool$. A number of factors may account for this: communist activities were strongly repressed in Shanghai during the 1930s and the Communist Party itself was underground, making our dependent variable particularly noisy. Building on these arguments, we conservatively interpret our estimates as a lower bound on the relationship between the Silver Purchase shock and Communist Party penetration.

In sum, we find a robust impact of the Silver Purchase shock on labor relations and the likelihood of labor unrest episodes. We also find an impact on underground communist activities consistent with the labor unrest effect; however, in this case the estimated effect is weaker.

[29] Data on the duration of labor unrest episodes is only available for Shanghai, which restricts our sample. In unreported tests, we find a negative, but statistically not significant, relation between silver reserves and labor unrest episode duration. We omit these tests to conserve space; but they are available upon request.

## 1.5. Alternative explanations and discussion

In this section, we present tests to rule out alternative interpretations, and we discuss the external validity of our findings. First, we compare industries exposed to and isolated from world markets, as a check for mechanical exchange rate effects and the impact of the worldwide Great Depression. Second, we present falsification tests based on a time period that precedes the silver shock and on data from neighboring Hong Kong, where banks did not face a mandatory reserve requirement to issue banknotes. Third, we show that the labor unrest episodes examined in the previous section are unlikely driven by the 1932 "Shanghai incident." Fourth, we use instrumental variables estimation to rule out effects due to self-selection of banks into high- and low-silver reserves groups. Fifth, we document the impact of the credit shock on output, for a set of firms for which good quality output data survive. Finally, we briefly discuss the external validity of our findings.

*A. Exchange rates and the Great Depression*

The Silver Purchase program is announced in the midst of the Great Depression. The global decline in demand, thus, could affect Chinese firms. Moreover, because of the silver standard, the rise in silver prices leads to an appreciation of the Chinese dollar, hurting the competitiveness of Chinese exporters. Both effects may predict a generalized contraction of Chinese credit demand.

But neither mechanism appears, in fact, to account for our findings. First, it is simply not clear why their effects should be more pronounced on banks with lower silver reserves, or the firms that borrow from them. Second, the loan-level estimates control for firm fixed effects, which capture firms' credit demand. As a third argument against the alternative interpretation, we present further evidence by comparing firms with different exposure to international trade, splitting industries between Traded and Non-Traded sectors. Both mechanisms outlined above predict stronger effects in the Traded sector.

Table 7 presents the estimates. We find similar results as in the overall sample: Banks with lower pre-1933 silver reserves reduce lending to firms in both Traded and Non-Traded sectors (columns (1)-(3)). The coefficient estimate on the interaction term between 1931 silver reserves and the Traded sector indicator is insignificant in all three specifications. When looking at social unrest (columns (4)-(9)), we also find similar results as in the overall sample; and in fact the effects are, if anything, *weaker* for Traded-sector firms, contradicting the alternative argument.

These findings suggest that the exchange rate and Great Depression channels are unlikely driving our results.

They also alleviate a remaining potential issue with the identification approach in our loan-level tests. As Khwaja and Mian (2008), we rely on the fact that credit demand is unrelated to the shock to credit supply, i.e. to the banks' silver reserves. Our results indicate that a major driver of credit demand, namely exposure to international trade, is essentially unrelated to our main effect. This is therefore consistent with our findings being attributable to credit supply.

*B. Falsification tests; the Shanghai incident*

We run two falsification tests. The first one provides additional evidence that no omitted factor related to the Chinese economy drives our results. To construct it, we look at bank lending between 1927 and 1931, before the Silver Purchase Program was enacted. We set up an "artificial" shock in 1929, and estimate equation (1) on the banks for which balance sheet data are available between 1927 and 1931, with the same treatment variable as in the previous tests (i.e. 1931 silver reserves). We present the results in Table 8. Across all specifications, there appears to be no significant relationship between silver reserves and the change in lending around 1929. Importantly, the implied effects are also economically very small, ranging between one-tenth and one-third of the effects from Table 3 (in addition, in columns (5)-(6), the sign of the coefficients is opposite to the estimates of Table 3).

The second falsification test aims to rule out a spurious correlation with economic events that may have affected the whole East Asia, other than the Silver Purchase program. To construct it, we focus on Hong Kong, as the economy closest to China in geographic and cultural terms. We rely on data on lending from the archives of HSBC, the main lender in the city-state at the time.[30] In the 1930s, Hong Kong is also on a silver standard: only silver coins have legal tender, and only minted silver can be used to pay taxes. Banks are allowed to issue banknotes, but these do not have the status of legal tender and, crucially, they are not required to be backed by silver reserves. Since the legal reserve requirement is the key driving force behind the credit contraction in 1930s mainland China, we should expect no such contraction in Hong Kong. We use lending amounts in HSBC's balance sheets, standardized by total assets and deposits, and track them over the period 1931-1935. We find that the average loans-to-deposits ratio is about 50%, both before and after

[30] In 1931, banknotes issued by HSBC correspond to 82% of the total banknotes issued in Hong Kong.

1933. Similarly, the loans-to-assets ratio is about 37%, both before and after 1933. In sum, we find that credit does not contract in Hong Kong, unlike in the Republic of China.

Finally, we verify that Japanese influence in parts of Shanghai does not drive our labor unrest and communist penetration results. This possibility relates to the so-called Shanghai incident where, in January 1932, the Japanese secret service staged a beating of Japanese Buddhist monks to justify military action against China. Japanese influence on Chinese firms concentrated in the Zhabei (闸北) district in Shanghai. Excluding Zhabei district firms from our sample does not materially alter our findings. These results are reported in Appendix Tables C.8 (labor unrest) and C.9 (communist penetration).

*C. Selection into high- and low-reserves groups and instrumental variables estimation*

A further challenge might be that silver reserves are not randomly assigned to banks. In principle at least, they may be correlated with unobserved factors, related e.g. to the banks' clientele and/or business model, affecting lending policies and the probability of labor unrest. Our results so far considerably raise the bar for a "selection" explanation of this sort. Since we observe *changes* in credit and labor unrest intensity after 1933, whatever unobserved sorting variable may drive our results must change precisely around the start of the Silver Purchase program, and must not be captured by the firm fixed effects tests reported in Table 4.

To further alleviate concerns about selection, we resort to instrumental variables estimation. We exploit a unique feature of 1930s China's monetary system: the parallel circulation of a traditional currency, issued by the "native" banks, and backed by copper instead of silver. The use of copper as a monetary base dates back to about 1100 BC (Kann (1927, pp. 403-404)). In the 1930s, copper-backed money circulates only locally, and it is mainly used to clear small transactions (Tamagna (1942, p. 68)). It is, however, not a trivial quantity: Rawski's (1989, p. 394) estimates indicate that it corresponds to about 13% of the silver-backed monetary base at the beginning of 1931. Moreover, this is a national average that hides local differences. Anecdotal evidence suggests that copper currency circulation was considerably more widespread in Shandong, Jiangsu, Guangdong, and Hebei, provinces that are relatively closer to copper mines, vis-à-vis other provinces where the use copper money was less common (Tamagna (1942, p. 68)).

We conjecture that the relative abundance of copper at mines near the headquarters of a given *modern* bank determines the availability of copper-backed currency. Further, if people use the copper-backed currency as a (partial) substitute for Chinese silver dollars, greater availability

of copper should associate with lower silver reserves, as the modern bank may face a lower demand for silver-backed currency for transaction purposes.

We build an index of the local $Copper\ pool$ in the same spirit as $Silver\ pool$ defined above, as the natural logarithm of the inverse distance-weighted-average copper capacity in the three copper mines nearest to a given bank. To the extent that the availability of copper mines near a given bank's headquarters is exogenous to credit provision and to social unrest *in Nanjing, Shanghai, and Tianjin* (which are generally far from the bank's headquarters), our instrument meets the exclusion restriction.

The first-stage regressions reported in Appendix Table C.3 validate our conjecture, and the relevance of the instrument: where copper abounds, banks hold smaller silver reserves. The Kleibergen-Paap first-stage test statistic is also large, suggesting that our instrument is not weak.

Table 9.A presents the second-stage estimates. They confirm our earlier findings: banks with lower 1931 silver reserves reduce credit provision after the 1933 Silver Purchase shock. In Table 9.B, we present the results for social unrest, defining an index of the available copper pool around a given firm in a way analogous to the previous index. The second-stage regressions are presented in Table 9.B for social unrest intensity (log-number of labor unrest episodes, and communist activities indicator).[31] A larger pool of available silver reserves at the firm's lenders is, again, negatively associated with the labor unrest episodes and communist activities. indicator. Overall, these tests suggest that selection is not a main driver of our results.[32]

*D. Layoffs, energy consumption, and output of cotton mills*

Figure 4 shows that the most common reasons for labor unrest episodes are layoffs and salary cuts, consistent with those episodes being driven by a contraction in investment and output at the firm level. A direct test of the impact of the Silver Purchase shock on output is limited by the availability of firm-level output data. Good quality information is available from Kraus (1980) on employment, energy consumption, and output for a sample of 38 cotton mills located in Shanghai and Tianjin.

[31] Again, to conserve space we relegate a corresponding test looking at the duration of labor unrest episodes to Appendix Table C.2.

[32] The second-stage estimates are somewhat larger than the OLS estimates. One possible explanation is that banks' silver reserves correlate with the investment opportunities of the firms they lend to. This suggests that the potential endogeneity actually works against us finding any effects, to the extent that banks have lower excess silver reserves because they lend more aggressively to firms with greater investment opportunities. As a result, credit demand should be stronger for banks with lower reserves, working against our main effect.

Using this information, we relate the probability that firms lay off workers, reduce electricity consumption (measured in kilowatts), and cut output (measured in standard units of pieces of cloth) to our measures of silver availability. We present the results in Table 10.[33] Across all specifications, the estimates document that firms with access to a smaller pool of silver reserves are more likely to curb employment and reduce energy consumption and textile output. In economic terms, the effects are sizable: firms with access to the lowest reserves pool are 48% more likely to curb employment, 60% more likely to reduce electricity consumption, and about 30% more likely to reduce output.

*E. External validity*

Because our results are based on data from 1930s China, it is important to discuss the extent of their external validity. That depends on (i) whether modern credit shocks can be similar in magnitude to the Chinese one of 1933-35, and (ii) whether a modern society will respond similarly to a shock of similar size.

The answer to the first question is yes. Chinese credit-to-GDP drops by about 15% over 1933-35 (Cheng (2003, Appendix II), Liu (1946, Table 1), and Figure 1). This value is very close to the drops experienced by Finland in 1991 (13%), Turkey in 2000 (18%), Spain in 2010 (10%), all identified by Laeven and Valencia (2012) as recent banking crises.[34]

As for the second question, one key ingredient to the answer is labor mobility. If workers are able to move and seek employment at firms that are not credit-constrained, a shock such as the Silver Purchase program should generate less social tension in a modern economy. Labor mobility in 1930s China, however, is not especially limited compared to modern China. Regulatory restrictions on internal migration, in fact, are less stringent in 1931-35 than today.[35] Moreover, internal migration is not uncommon in 1930s China. In 1935, immigration inflow (outflow) as a

[33] The dependent variables are indicators that take the value of 1 if the firm reduces employment (Panel A), electricity consumption (Panel B), output (Panel C) by more than 2% from the previous year, and 0 otherwise. The 2% cutoff limits the impact of noise; more conservative cutoffs deliver even stronger results.

[34] Data on credit-to-GDP for these episodes are retrieved from the World Bank's website.

[35] According to the *Household Registration Law* (户籍法), passed in 1931, individuals can register at a new location after living there for 6 years, and there is no further discrimination between temporary and permanent residents. In contrast, formally registering as a Shanghai resident in the 2010s requires at least 7 years of residence, minimum professional qualifications and social security contributions; temporary residents, moreover, face severe welfare discrimination (Li and Ren (2011)).

fraction of total population is 16% (13%), quite comparable with the 1995-2000 average of 13% (10%).[36]

Of course, this conclusion must be moderated by a number of other factors beyond the scope of our analysis, such as the government's willingness and ability intervene to attenuate the negative effects of economic shocks.

## 1.6. Conclusion

Using a novel, hand-collected dataset and a natural experiment from 1930s China, we provide new micro-econometric evidence on the socio-political consequences of negative economic shocks. We exploit the 1933 U.S. Silver Purchase program, which, raising silver prices worldwide, generates a credit contraction in the Republic of China, on the silver standard in the 1930s. We find that (i) banks more exposed to the shock (with lower pre-1933 silver reserves) cut lending after 1933, and (ii) labor unrest episodes and Communist Party penetration at firms borrowing from exposed banks increase in intensity after 1933, relative to other firms. The level of detail of our micro-data helps a causal interpretation of the findings, addressing the reverse causality problem that often characterizes tests of the social impact of economic shocks. Our evidence is consistent with credit rationing (and more in general, economic shocks) having a material impact on social unrest.

[36] Total Shanghai population for 1935 is retrieved from *Statistics Monthly. No 32.* (统计月报； 32 号), compiled by Statistics Bureau of the Guangxi (广西省政府统计局). Immigration inflow and outflow are obtained from *Police Monthly* No. 3, Roll 3 (警察月刊;第三期第三卷), compiled by the Shanghai Police Bureau (上海市公安局第一科). Data for modern Shanghai are obtained from Fan (2005), and from the 5th National Population Census of the People's Republic of China.

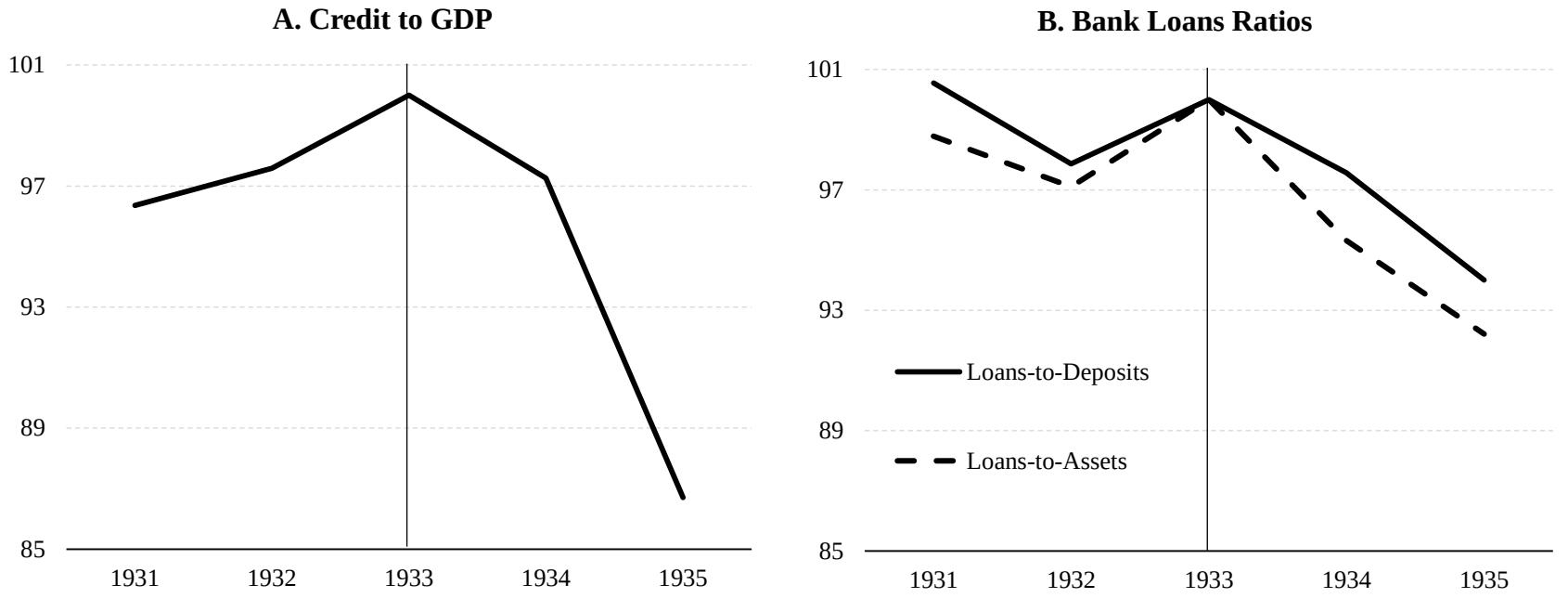


**Figure 1 Aggregate Credit in China, 1931-1935**

The graphs report measures of aggregate credit provision in the Republic of China over the sample period. Panel A plots the total credit-to-GDP ratio for the Republic of China, rescaled so as to take the value of 100 in 1933. Total credit is the aggregate of the loans data reported on bank balance sheets, used throughout; GDP is based on manufacturing output from factories (Brandt and Sargent (1989), Table 5). Panel B plots two additional ratios, again for the Republic of China: total loans-to-deposits and total loans-to-assets. Total loans are obtained as the aggregate of the loans data reported on bank balance sheets, used throughout; and deposits and total assets data are retrieved from Cheng (2003), Appendix II.

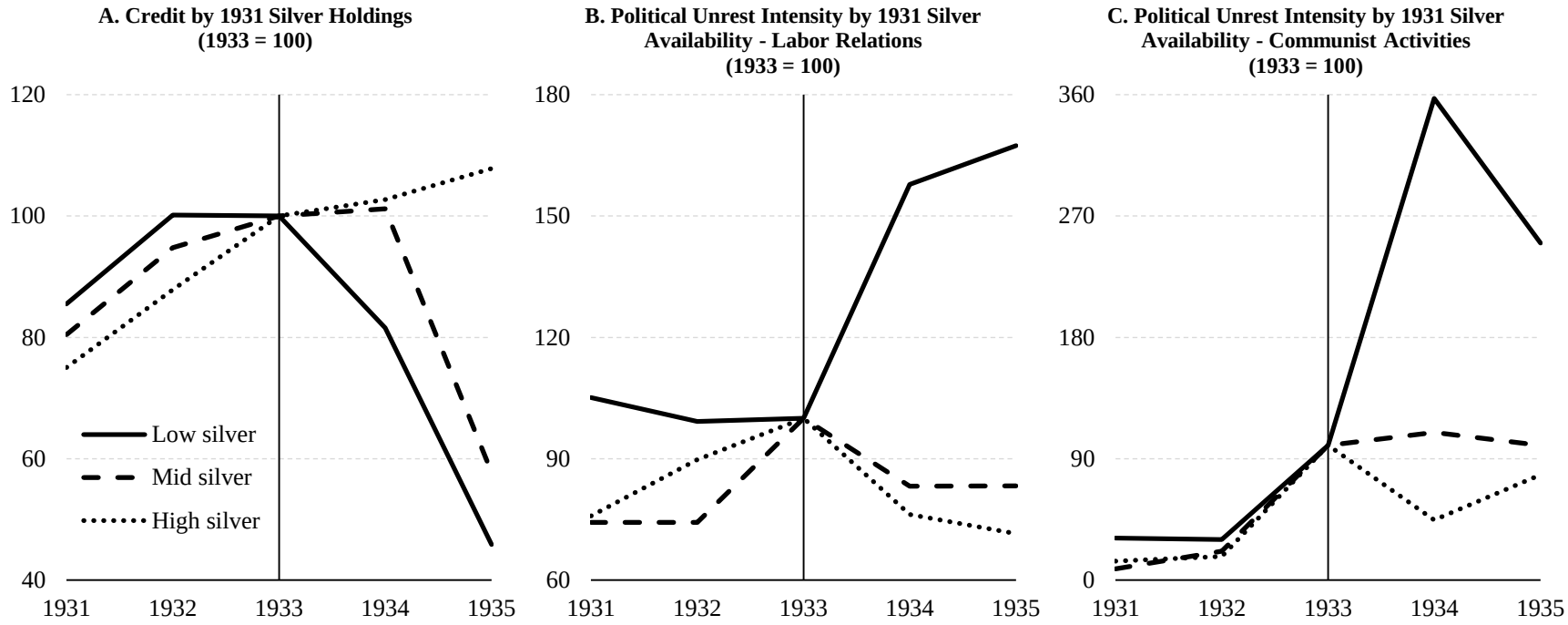


**Figure 2 Chinese Credit and Political Unrest Intensity, 1931-1935**

Panel A reports the Chinese banks' aggregate dollar lending over the 1931-1935 period, broken down into components due to banks with low (below the 25th percentile), high (above the 75th percentile), and medium *Excess silver*. All series are normalized such that the 1933 value equals 100. The credit data are based on the bank balance sheet data discussed in Section IV.A. Panel B plots the average number of labor unrest episodes per plant, and panel C plots the number of plants in which support for communist activities was detected, as a fraction of membership of the communist-sponsored union (Shen (1987, p.143) and Stranahan (1998, p.154)), in both cases based on the level of silver holdings available at banks in the plants' vicinity, *Excess silver pool*, as described in Section IV.B, again normalized such that the 1933 value equals 100. The figures show that, after the Silver Purchase program is initiated, banks with lower excess silver holdings in 1931 reduce lending relative to banks with larger holdings. Furthermore, firms and plants with a greater exposure to the Silver Purchase shock – in the vicinity of banks with lower excess silver reserves – experience intensified labor unrest episodes and increased support for communist activities after 1933.

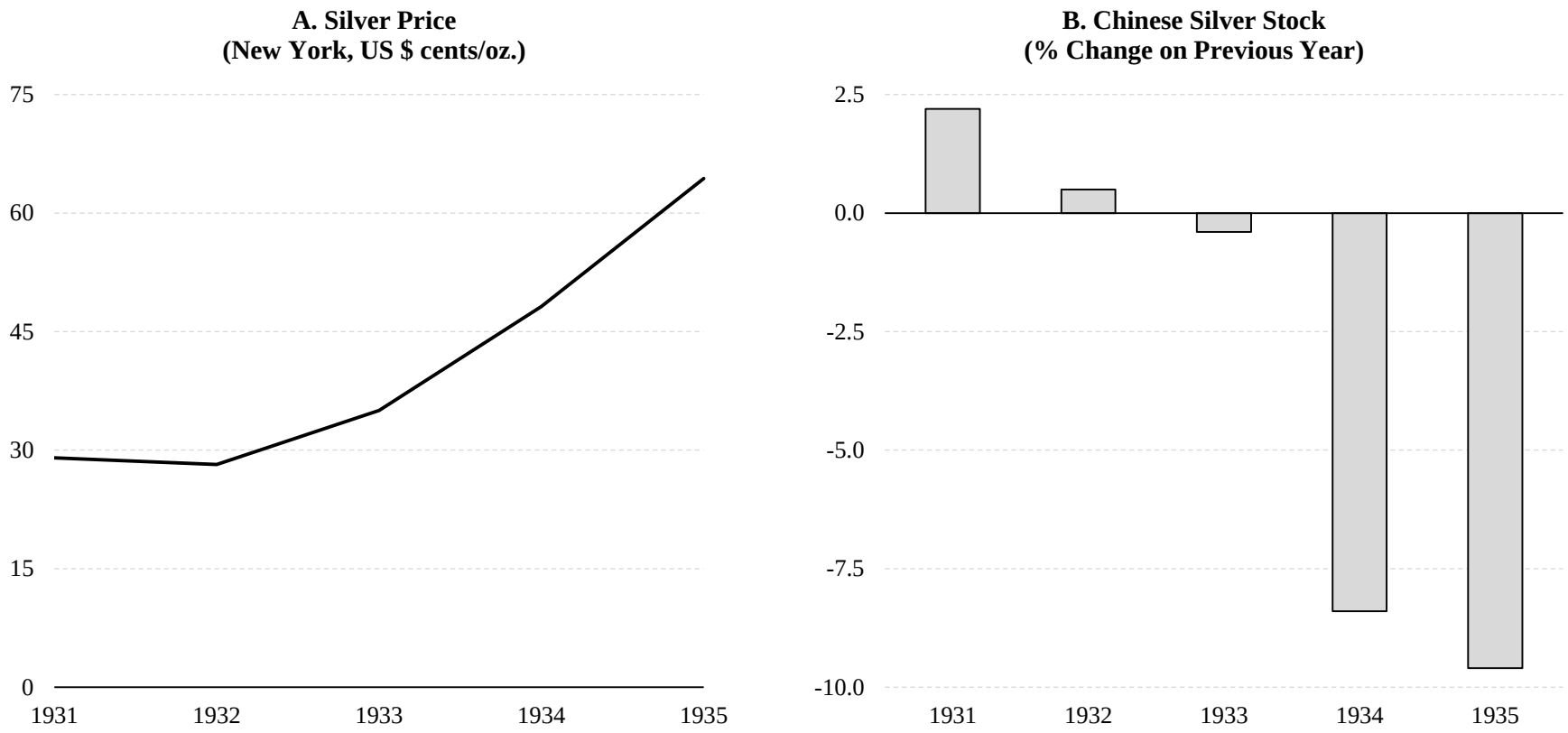


**Figure 3 Silver Prices and Changes in Chinese Silver Stock, 1931-1935**
Panel A reports the silver price quotes in New York over the period 1931-35 (source: *Economy Xun Kan* (经济旬刊); Vol. 4, No. 13, p. 11). Panel B reports yearly changes in the Chinese stock of silver reserves (source: Rawski (1989, p.394)).

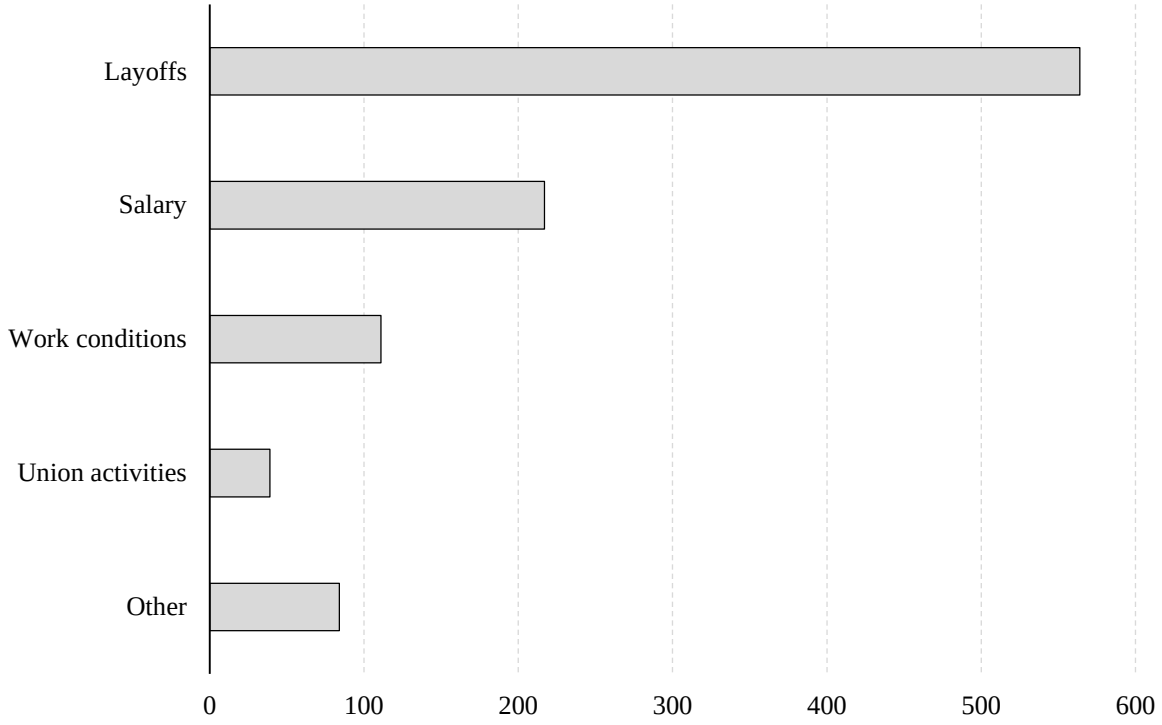


**Figure 4 Causes of Labor Unrest Episodes, 1931-1935**

The figure reports the causes of labor unrest episodes taking place in Shanghai during the period 1931-1935, corresponding to Table 2.B. The labor unrest data are retrieved from surveys *Strikes and lockouts in Shanghai since 1918* (近十五年来上海之罢工停业) (1931-32), *Strikes and lockouts in Shanghai in the Past Four Years* (近四年来上海的罢工停业) (1933-35), *Industrial Disputes in Shanghai since 1928* (近五年来上海之劳资纠纷) (1931-32), conducted by the Bureau of Social Affairs of the city government of greater Shanghai, and the survey *Industrial Disputes in Shanghai in the Past Four Years* (近四年来上海的劳资纠纷), published by the *International Labor Bulletin* (1933-35).

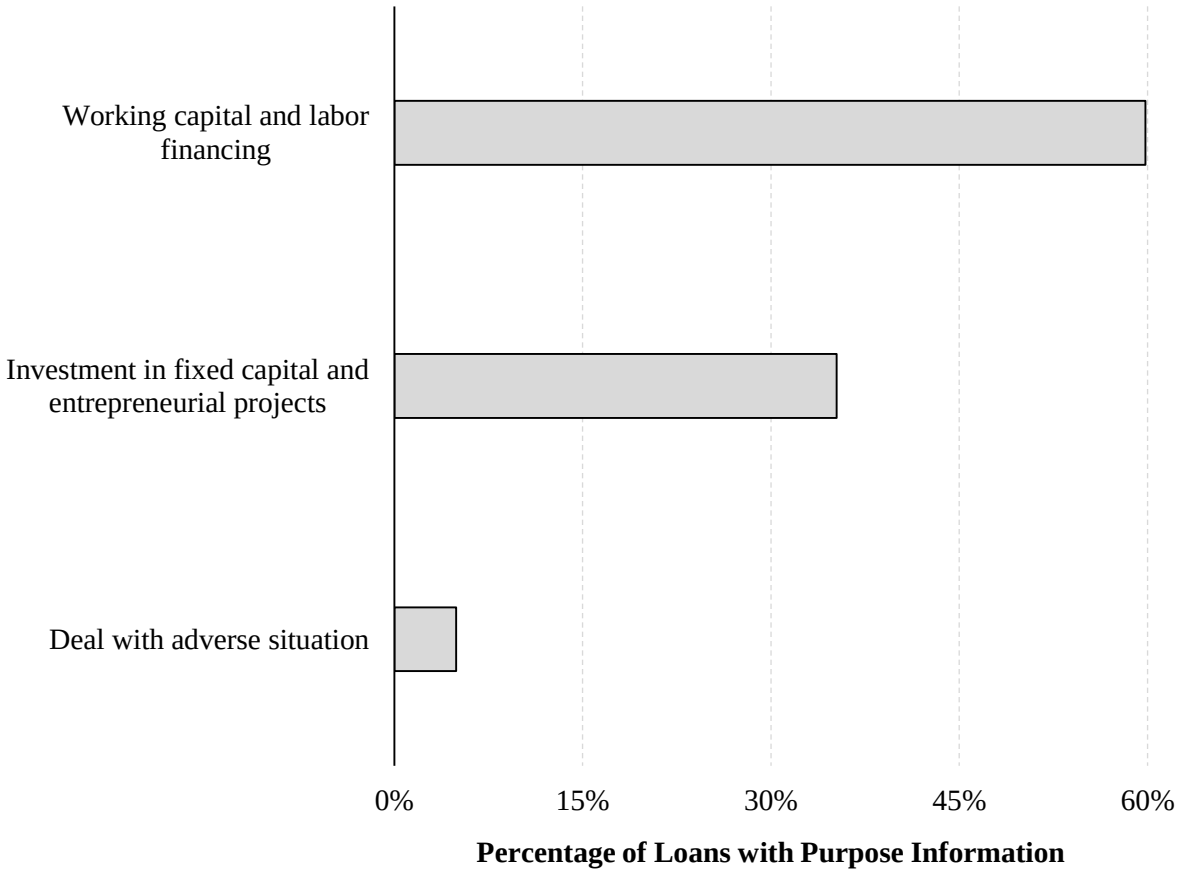


**Figure 5 Loan Purpose, 1931-1935**
The figure reports the frequency of loan purposes in our sample, as a percentage of the number of loans with available purpose information. There are 244 loans with available loan purpose information. The sample comprises the set of loan contracts described in section III.A, with available loan purpose information.

**Table 1 Bank Balance Sheets and Loans – Summary Statistics**

Panel A reports summary statistics on balance sheet data for the banks in our sample. All figures are expressed in thousands of Chinese dollars. Panel B provides summary statistics for the loan contracts in our sample. Loan amounts are expressed in thousands of Chinese dollars. The number of loans granted to each industry is counted based on observations with identifiable industry information, according to the International Labor Organization (1923) classification. The data are hand-collected, and retrieved from a number of archival sources described in greater detail Section III.

**A. Bank Level Variables (Chinese $000)**

| | N | Mean | St. dev. | Min | Max |
|---|---|---|---|---|---|
| Total assets | 523 | 36,151 | 118,324 | 67 | 1,342,242 |
| Equity | 527 | 2,976 | 7,839 | 23 | 103,845 |
| Cash | 519 | 2,647 | 8,410 | 0 | 87,409 |
| Total loans | 517 | 17,193 | 46,475 | 34 | 509,600 |
| Notes issuance | 494 | 4,737 | 15,504 | 0 | 161,000 |
| Silver reserves | 366 | 3,728 | 12,515 | 0 | 129,000 |
| Deposits | 508 | 25,704 | 86,457 | 9 | 992,941 |
| Net income | 517 | 301 | 1,093 | -291 | 14,822 |
| 1931 Silver reserves | 505 | 1,932 | 7,205 | 0 | 64,440 |
| 1931 Excess silver reserves | 505 | 229 | 1,194 | 0 | 10,722 |
| 1931 Excess reserves | 505 | 0.19 | 0.39 | 0 | 1 |
| Return on equity (%) | 517 | 10 | 9 | -38 | 63 |
| Cash to assets ratio (%) | 519 | 7 | 8 | 0 | 59 |
| Equity to assets ratio (%) | 522 | 21 | 16 | 1 | 91 |
| No retained earnings | 527 | 0.09 | 0.28 | 0 | 1 |
| No notes banks | 690 | 0.37 | 0.48 | 0 | 1 |

**B. Loan Contract Characteristics**

| | N | Mean | St. dev. | Min | Max |
|---|---|---|---|---|---|
| Loan Amount (Chinese $000) | 579 | 273 | 758 | 0.40 | 9,000 |

**Loan Contracts by Industry**

| | | | | | |
|---|---|---|---|---|---|
| Transportation | 145 | Chemistry | 13 | Construction | 1 |
| Services | 125 | Retail | 9 | Wood products | 1 |
| Textiles | 68 | Agriculture | 8 | Other | 1 |
| Power & utilities | 28 | Finance | 6 | | |
| Specials (hospital, school, etc.) | 23 | Glass products | 4 | | |
| Mining | 21 | Machinery | 3 | | |
| Food | 18 | Paper products | 2 | | |

**Table 2 Political Unrest – Summary Statistics**

The table reports summary statistics on political unrest from Shanghai, Tianjin, and Nanjing. Panel A summarizes first the political unrest intensity in terms of the number of labor unrest episodes, the duration of the episodes, and if communist activities are detected at a given firm. Information on the available silver pool for a given firm is also reported. For labor unrest episode duration and communist activities, due to data availability the sample is restricted to Shanghai firms. Panel B reports a breakdown of the causes of labor unrest cases in Shanghai. The labor unrest episodes data in Shanghai are retrieved from surveys *Strikes and lockouts in Shanghai since 1918* (近十五年来上海之罢工停业) (1931-32), *Strikes and lockouts in Shanghai in the Past Four Years* (近四年来上海的罢工停业) (1933-35), *Industrial Disputes in Shanghai since 1928* (近五年来上海之劳资纠纷) (1931-32), conducted by the Bureau of Social Affairs of the city government of greater Shanghai, and the survey *Industrial Disputes in Shanghai in the Past Four Years* (近四年来上海的劳资纠纷), published by the *International Labor Bulletin* (1933-35). The labor unrest episodes data in Tianjin are hand collected from several newspapers described in the text. The labor unrest episodes data in Nanjing are retrieved from surveys *Industrial Disputes in Nanjing 1932-1934* (南京市之劳资纠纷统计) and *Industrial Disputes in Nanjing 1935* (民国二十四年南京市劳资纠纷统计), published by the *Labor Monthly* journal (劳工月刊). The underground communist activities record among Shanghai firms are collected from the SMP files, as described section III.

**A. Political Unrest and Access to Silver Reserves**

| | N | Mean | St. dev. | Min | Max |
|---|---|---|---|---|---|
| No. of labor unrest episodes | 8,632 | 0.14 | 0.56 | 0 | 10 |
| Duration of labor unrest episodes (Shanghai) | 7,500 | 2.82 | 14.65 | 0 | 319 |
| Communist activities Y/N (Shanghai) | 7,500 | 0.02 | 0.14 | 0 | 1 |
| 1931 Silver pool (Chinese $000) | 8,715 | 7,616 | 2,133 | 5,487 | 15,788 |
| 1931 Excess silver pool (Chinese $000) | 8,715 | 912 | 320 | 622 | 2,124 |
| 1931 Excess reserves pool | 8,715 | 0.47 | 0.09 | 0.38 | 0.82 |

**B. Causes of Labor Unrest (for Shanghai)**

| Causes | No. Episodes |
|---|---|
| Layoffs | 564 |
| Salary | 217 |
| Work conditions | 111 |
| Union activities | 39 |
| Other | 84 |
| Total | 1,015 |

**Table 3 Silver Reserves and Credit around 1933 – Bank-Level**
The table reports the estimates of:

$$\Delta L_b = \delta + \gamma Silver_{b,1931} + \mu' \Delta x_b + \varepsilon_b$$

The dependent variable is the change in the natural logarithm of the overall loans ($Total\ loans$) extended by bank $b$ around 1933 (post-1933 average minus pre-1933 average). The variable $Silver$ is the natural logarithm of bank's silver reserves in 1931 (or the value in 1932 if not available, specifications (1)-(3)). We alternatively define it as the log-amount of silver reserves in excess of the mandatory 60% reserves (specifications (4)-(5)), or as an indicator variable equal to 1 if a given bank has silver-to-notes ratio strictly above 60%, and 0 otherwise (specifications (6)-(7)). $x$ is a vector of control variables, including bank size, equity ratio, cash to bank assets ratio, return on equity, and an indicator for no retained earnings. Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around 1933, after collapsing and time-averaging the data before and after 1933. Specifications (3), (5) and (7) also include banks that do not issue banknotes throughout the sample period (and add an indicator for no notes issuance). All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Silver | 0.124*** | 0.125** | 0.129*** | | | | |
| | (0.039) | (0.047) | (0.044) | | | | |
| Excess silver | | | | 0.391*** | 0.400*** | | |
| | | | | (0.073) | (0.070) | | |
| Excess reserves (Y/N) | | | | | | 0.130*** | 0.133*** |
| | | | | | | (0.041) | (0.041) |
| Δ Bank size | | 0.076 | 0.048 | 0.074 | 0.045 | 0.108 | 0.085 |
| | | (0.087) | (0.073) | (0.095) | (0.087) | (0.084) | (0.076) |
| Δ Equity ratio | | -0.006 | -0.005*** | -0.005 | -0.004** | -0.005 | -0.005*** |
| | | (0.004) | (0.002) | (0.004) | (0.002) | (0.003) | (0.002) |
| Δ Cash ratio | | -0.002 | -0.003 | -0.002 | -0.003 | -0.003 | -0.003 |
| | | (0.003) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Δ ROE | | 0.004 | 0.003* | 0.005* | 0.004** | 0.004* | 0.004** |
| | | (0.003) | (0.002) | (0.003) | (0.002) | (0.002) | (0.002) |
| Δ No ret. earnings | | 0.045 | 0.015 | 0.079 | 0.038 | 0.063 | 0.028 |
| | | (0.082) | (0.054) | (0.077) | (0.052) | (0.068) | (0.048) |
| No notes | | | -0.015 | | -0.027 | | 0.007 |
| | | | (0.031) | | (0.031) | | (0.031) |
| Intercept | 0.034 | 0.020 | 0.018 | 0.042 | 0.038* | 0.002 | -0.001 |
| | (0.022) | (0.029) | (0.024) | (0.026) | (0.022) | (0.024) | (0.021) |
| N | 47 | 46 | 80 | 46 | 80 | 46 | 80 |
| $R^2$ | 0.12 | 0.30 | 0.33 | 0.30 | 0.33 | 0.40 | 0.40 |

**Table 4 Silver Reserves and Credit around 1933 – Loan-Level**
The table reports the estimates of:

$$\Delta L_{bf} = \alpha_f + \gamma Silver_{b,1931} + \mu' \Delta x_{bf} + \varepsilon_{bf}$$

The dependent variable is the change in the natural logarithm of loans ($Loan\ amount$) extended by bank $b$ to firm $f$ (average after 1933 minus average prior to 1933). The variable $Silver$ is the natural logarithm of the bank's silver reserves in 1931 (or the value in 1932 if not available (specifications (1) and (4)), the log-amount of silver in excess to compulsory reserves (specifications (2) and (5)), or an indicator variable equal to 1 if a given bank has silver-to-notes ratio above 60%, and 0 otherwise (specifications (3) and (6)). $x$ is a vector of control variables. In addition, columns (4)-(6) include borrowing firm fixed effects. Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around 1933, after collapsing and time-averaging the data before and after 1933. We focus on banks that issue banknotes. All variables are defined in detail in Appendix B. The standard errors, clustered by bank, are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | OLS | | | FE | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver | 0.121** | | | 0.176*** | | |
| | (0.049) | | | (0.057) | | |
| Excess silver | | 0.078*** | | | 0.100*** | |
| | | (0.020) | | | (0.031) | |
| Excess reserves (Y/N) | | | 0.282* | | | 0.309** |
| | | | (0.149) | | | (0.150) |
| Δ Bank size | 0.011 | 0.201 | 0.159 | 0.357 | 0.660** | 0.558 |
| | (0.519) | (0.440) | (0.511) | (0.260) | (0.292) | (0.344) |
| Δ Equity ratio | -0.059 | -0.026 | 0.006 | -0.018 | 0.039 | 0.082 |
| | (0.052) | (0.046) | (0.047) | (0.039) | (0.035) | (0.058) |
| Δ Cash ratio | -0.034*** | -0.033*** | -0.035*** | -0.016** | -0.015* | -0.016* |
| | (0.008) | (0.007) | (0.008) | (0.007) | (0.007) | (0.008) |
| Δ ROE | -0.009 | 0.009 | 0.018 | -0.011 | 0.015 | 0.027 |
| | (0.029) | (0.024) | (0.029) | (0.014) | (0.013) | (0.019) |
| Δ No ret. earnings | -0.387 | -0.120 | -0.134 | -0.442 | -0.147 | -0.202 |
| | (0.328) | (0.357) | (0.401) | (0.281) | (0.248) | (0.303) |
| Intercept | -0.491 | 0.080 | 0.127 | -0.732* | 0.146 | 0.266 |
| | (0.390) | (0.161) | (0.159) | (0.411) | (0.144) | (0.174) |
| Firm f.e. | N | N | N | Y | Y | Y |
| N | 306 | 306 | 306 | 229 | 229 | 229 |
| $R^2$ | 0.02 | 0.02 | 0.02 | 0.84 | 0.84 | 0.83 |

**Table 5 Impact of the Liquidity Shock on Social Unrest – Labor Relations**

Panel A reports the estimates of:

$$\Delta Labor\ unrest_f = \delta + \gamma Silver\ pool_f + \mu' x_f + \varepsilon_f$$

The dependent variable is either the change in the natural logarithm of the number of labor unrest episodes at firm $f$ in columns (1)-(3) (average after 1933 minus average prior to 1933), or the change in the natural logarithm of the duration of unrest episodes in firm $f$ in columns (4)-(6). The variable S$ilver\ pool$ is the inverse distance-weighted average silver reserves around firm $f$ (columns (1) and (4)), the inverse distance-weighted average of the silver amount in excess of the 60% minimum reserve requirement, in columns (2) and (5) ($Excess\ silver\ pool$), or the inverse distance-weighted average of the indicator variable taking the value of 1 if a given bank has silver-to-notes ratio above 60%, and 0 otherwise, in columns (3) and (6) ($Excess\ reserves\ pool$). All specifications include city district, industry, and firm nationality fixed effects. In panel B, column (1), we report the estimates of:

$$Relation_{fb} = \alpha + \beta Distance_{fb} + \varepsilon_f$$

The variable $Relation$ is an indicator variable taking the value of 1 if a given bank-firm pair has a lending relationship, and 0 otherwise. The variable $Distance$ is the natural logarithm of 1 plus the distance (measured in km) between a given bank-firm pair. In columns (2)-(4), the sample is restricted to bank-firm pairs where we observe a lending relationship, and we directly link labor unrest at the firms to their bank lenders' silver holdings. All specifications include city fixed effects. Following Bertrand, Duflo, and Mullainathan (2004), specifications (1)-(3) are estimated on changes around 1933. All variables are defined in detail in Appendix B. Standard errors reported in the parentheses in panel B, column (1), are clustered by firm. The Huber-White robust standard errors are reported in parentheses. The symbols *, **, and*** denote statistical significance at the 10%, 5%, and 1% levels respectively.

**A. Full sample**

| **Dep. variable:** | **$\Delta\log(1 + Number\ of\ labor\ unrest\ episodes)$** | | | **$\Delta\log(1 + Duration)$** | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver pool | -0.246*** | | | -0.611** | | |
| | (0.065) | | | (0.244) | | |
| Excess silver pool | | -0.229*** | | | -0.602*** | |
| | | (0.056) | | | (0.207) | |
| Excess reserves pool | | | -0.675*** | | | -1.326* |
| | | | (0.208) | | | (0.783) |
| District f.e. | Y | Y | Y | Y | Y | Y |
| Industry f.e. | Y | Y | Y | Y | Y | Y |
| Nationality f.e. | Y | Y | Y | Y | Y | Y |
| N | 1,743 | 1,743 | 1,743 | 1,500 | 1,500 | 1,500 |
| $R^2$ | 0.12 | 0.12 | 0.12 | 0.08 | 0.08 | 0.08 |

**Table 5 Impact of the Liquidity Shock on Social Unrest - Labor Relations; continued**

**B. Existing bank-firm relationships**

| **Dep. variable:** | **Relation (Y/N)** | $\Delta\log(1 + Number\ of\ labor\ unrest\ episodes)$ | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Distance | -0.018** | | | |
| | (0.007) | | | |
| Silver | | -0.027* | | |
| | | (0.014) | | |
| Excess silver | | | -0.024* | |
| | | | (0.013) | |
| Excess reserves (Y/N) | | | | -0.180** |
| | | | | (0.085) |
| City f.e. | Y | Y | Y | Y |
| N | 1,899 | 60 | 60 | 60 |
| $R^2$ | 0.01 | 0.09 | 0.09 | 0.13 |

**Table 6 Impact of the Liquidity Shock on Social Unrest – Communist Activities**
The table reports the estimates of:

$$\Delta Communist\ activities_f = \delta + \gamma Silver\ pool_f + \mu' x_f + \varepsilon_f$$

The dependent variable is the change in the natural logarithm of the number of communist activities mentioned in the SMP files at firm $f$ (average after 1933 minus average prior to 1933). Due to data availability, the sample contains Shanghai firms only. The variable S$ilver\ pool$ is the inverse distance-weighted average silver reserves around firm $f$ (specifications (1)-(3)), the inverse distance-weighted average of the log-amount of silver in excess to compulsory reserves, in specifications (4)-(6) ($Excess\ silver\ pool$), or the inverse distance-weighted average of the indicator variable equal to 1 if a given bank has silver-to-notes ratio above 60%, and 0 otherwise, in specifications (7)-(9) ($Excess\ reserves\ pool$). Specifications (1), (4) and (7) include firm nationality fixed effects. Specifications (2), (5) and (8) include industry, and firm nationality fixed effects. Specifications (3), (6) and (9) include city district, industry, and firm nationality fixed effects. Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around 1933, after collapsing and time-averaging the data before and after 1933. All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| **Dep. Variable: $\Delta\log(1 + Number\ of\ communist\ activities)$** | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Silver pool | -0.076** | -0.085** | -0.085* | | | | | | |
| | (0.031) | (0.035) | (0.044) | | | | | | |
| Excess silver pool | | | | -0.069** | -0.077** | -0.084** | | | |
| | | | | (0.028) | (0.030) | (0.036) | | | |
| Excess reserves pool | | | | | | | -0.167* | -0.185* | -0.156 |
| | | | | | | | (0.100) | (0.108) | (0.147) |
| Intercept | 0.494** | | | 0.300** | | | 0.070 | | |
| | (0.203) | | | (0.120) | | | (0.043) | | |
| District f.e. | N | N | Y | N | N | Y | N | N | Y |
| Industry f.e. | N | Y | Y | N | Y | Y | N | Y | Y |
| Nationality f.e. | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 1,500 | 1,500 | 1,500 | 1,500 | 1,500 | 1,500 | 1,500 | 1,500 | 1,500 |
| $R^2$ | 0.19 | 0.24 | 0.24 | 0.19 | 0.24 | 0.24 | 0.19 | 0.24 | 0.24 |

**Table 7 Traded vs. Non-traded Sectors: Credit and Social Unrest around 1933**

The table reports the estimates of regressions following specifications (4)-(6) of Table 4 in columns (1)-(3); specifications (1)-(3) of Table 5.A in columns (4)-(6); and specifications (3), (6) and (9) of Table 6 in columns (7)-(9). All specifications include the additional variables $Traded\ sector$ (an indicator equal to 1 for traded-products industrial sectors) and the interaction term $Traded\ sector \times Silver\ reserves$ (columns (1)-(3)) or $Traded\ sector \times Silver\ pool$ (columns (4)-(9)). Specifications (1)-(3) include a borrowing firm fixed effects. Specifications (4)-(9) include city district, industry and firm nationality fixed effects. Following Bertrand, Duflo, and Mullainathan (2004), all regressions are estimated on changes around 1933. All variables are defined in detail in Appendix B. The standard errors, reported in parentheses, are clustered by bank in columns (1)-(3), and Huber-White in columns (4)-(9). The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| **Dependent var.** | **$\Delta\log(1 + Loan\ amount)$** | | | **$\Delta\log(1 + Number\ of\ labor\ unrest\ episodes)$** | | | **$\Delta\log(1 + Number\ of\ communist\ activities)$** | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Silver | 0.183*** | | | -0.613*** | | | -0.305** | | |
| | (0.062) | | | (0.113) | | | (0.130) | | |
| Silver × | -0.041 | | | 0.437*** | | | 0.257* | | |
| Traded sector | (0.046) | | | (0.108) | | | (0.131) | | |
| Excess silver | | 0.103** | | | -0.569*** | | | -0.300** | |
| | | (0.038) | | | (0.101) | | | (0.124) | |
| Excess silver × | | -0.011 | | | 0.399*** | | | 0.249** | |
| Traded sector | | (0.046) | | | (0.098) | | | (0.126) | |
| Excess reserves (Y/N) | | | 0.275* | | | -1.370*** | | | -0.725** |
| | | | (0.148) | | | (0.327) | | | (0.320) |
| Excess reserves (Y/N) × | | | 0.397 | | | 0.876*** | | | 0.683** |
| Traded sector | | | (0.362) | | | (0.294) | | | (0.323) |
| Control var. | Y | Y | Y | | | | | | |
| Firm f.e. | Y | Y | Y | | | | | | |
| District f.e. | | | | Y | Y | Y | Y | Y | Y |
| Industry f.e. | | | | Y | Y | Y | Y | Y | Y |
| Nationality f.e. | | | | Y | Y | Y | Y | Y | Y |
| N | 229 | 229 | 229 | 1,743 | 1,743 | 1,743 | 1,500 | 1,500 | 1,500 |
| R2 | 0.84 | 0.84 | 0.83 | 0.14 | 0.14 | 0.13 | 0.24 | 0.25 | 0.24 |

**Table 8 Silver Reserves and Credit – Bank-Level Placebo Test (1927-1931)**
The table reports the estimates of:

$$\Delta L_b = \delta + \gamma Silver_{b,1931} + \mu' \Delta x_b + \varepsilon_b$$

The dependent variable is the change in the natural logarithm of the overall loans ($Total\ loans$) extended by bank $b$ around 1929 (post-1929 average minus pre-1929 average). The variable $Silver$ is the natural logarithm of bank's silver reserves in 1931 (or the value in 1932 if not available, specifications (1)-(3)). We alternatively define it as the log-amount of silver reserves in excess of the mandatory 60% reserves (specifications (4)-(5)), or as an indicator variable equal to 1 if a given bank has silver-to-notes ratio strictly above 60%, and 0 otherwise (specifications (6)-(7)). $x$ is a vector of control variables, including bank size, equity ratio, cash to bank assets ratio, return on equity, and an indicator for no retained earnings. Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around 1929, after collapsing and time-averaging the data before and after 1929. Specifications (3), (5) and (7) also include banks that do not issue banknotes throughout the sample period (and add an indicator for no notes issuance). All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Silver | 0.030 | 0.037 | | | | |
| | (0.088) | (0.084) | | | | |
| Excess silver | | | 0.054 | 0.114 | | |
| | | | (0.245) | (0.260) | | |
| Excess reserves (Y/N) | | | | | -0.012 | -0.019 |
| | | | | | (0.108) | (0.092) |
| Δ Bank size | 2.625** | 2.634*** | 2.657** | 2.672*** | 2.692** | 2.702*** |
| | (1.077) | (0.549) | (1.081) | (0.560) | (1.130) | (0.572) |
| Δ Equity ratio | -0.015 | -0.024* | -0.015 | -0.024* | -0.016 | -0.025* |
| | (0.014) | (0.013) | (0.014) | (0.013) | (0.015) | (0.013) |
| Δ Cash ratio | 0.008 | 0.000 | 0.008 | -0.000 | 0.008 | 0.000 |
| | (0.017) | (0.006) | (0.017) | (0.006) | (0.017) | (0.006) |
| Δ ROE | 0.016 | 0.007 | 0.016 | 0.007 | 0.016 | 0.007 |
| | (0.011) | (0.005) | (0.011) | (0.005) | (0.012) | (0.006) |
| Δ No ret. earnings | 0.058 | 0.024 | 0.063 | 0.031 | 0.066 | 0.033 |
| | (0.102) | (0.088) | (0.103) | (0.088) | (0.113) | (0.099) |
| No notes | | -0.116 | | -0.124 | | -0.143 |
| | | (0.083) | | (0.078) | | (0.085) |
| Intercept | 0.160** | 0.112 | 0.168** | 0.121* | 0.178* | 0.136 |
| | (0.074) | (0.068) | (0.070) | (0.062) | (0.089) | (0.083) |
| N | 21 | 27 | 21 | 27 | 21 | 27 |
| $R^2$ | 0.62 | 0.60 | 0.61 | 0.60 | 0.61 | 0.60 |

**Table 9 Credit and Social Unrest around 1933 – IV Estimation, 2nd Stage**
Panel A reports the estimates of:

$$\Delta L_{bf} = \alpha_f + \gamma Silver_{b,1931} + \mu' \Delta x_{bf} + \varepsilon_{bf}$$

following the same specifications as in columns (4)-(6) of Table 4. $Silver$ is instrumented by copper availability $Copper\ pool$, defined as the natural logarithm of distance-weighted average copper capacity in the closest 3 copper mines around bank $b$. $x$ is the vector of control variables used in Table 4. Panel B reports the estimates of:

$$\Delta Social\ unrest_f = \delta + \gamma Silver\ pool_f + \mu' x_f + \varepsilon_f$$

following the same specifications as in columns (1)-(3) of Table 5.A. $Silver\ pool$ is instrumented by copper availability $Copper\ pool$, defined as the inverse distance-weighted average of the natural logarithm of copper capacity in the nearest 3 copper mines to a firm's closest bank $b$. Columns (1)-(3) present the results for Δlog(1 + Number of unrest episodes), columns (4)-(6) present the results for Δlog(1 + Number of communist activities). All variables are time-averaged before and after 1933, and are defined in detail in Appendix B. The standard errors, reported in parentheses, are clustered by bank in panel A, and Huber-White in panel B. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| **A. Dep. variable: Δ$L$ (2nd stage)** | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Silver | 0.334*** | | |
| | (0.109) | | |
| Excess silver | | 0.301*** | |
| | | (0.113) | |
| Excess reserves (Y/N) | | | 0.864*** |
| | | | (0.298) |
| Control var. | Y | Y | Y |
| Firm f.e. | Y | Y | Y |
| N | 229 | 229 | 229 |
| First-stage F-stat | 10.20 | 3.88 | 17.69 |

| **B. Dep. variable: Δ$Social\ unrest$ (2nd stage)** | | | | | | |
|---|---|---|---|---|---|---|
| | Δlog(1 + $Number\ of\ labor\ unrest\ episodes$) | | | Δlog(1 + $Number\ of\ communist\ activities$) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver pool | -0.593** | | | -0.162 | | |
| | (0.273) | | | (0.155) | | |
| Excess silver pool | | -0.523** | | | -0.144 | |
| | | (0.242) | | | (0.138) | |
| Excess reserves pool | | | -1.874** | | | -0.495 |
| | | | (0.864) | | | (0.475) |
| District f.e. | Y | Y | Y | Y | Y | Y |
| Industry f.e. | Y | Y | Y | Y | Y | Y |
| Nationality f.e. | Y | Y | Y | Y | Y | Y |
| N | 1,743 | 1,743 | 1,743 | 1,500 | 1,500 | 1,500 |
| First stage F-stat | 132.82 | 132.31 | 105.14 | 129.75 | 124.33 | 115.40 |

**Table 10 Shanghai and Tianjin Cotton Mill Operation and Silver Availability**
The table reports the estimates of:

$$\Delta Operation_f = \delta + \gamma Silver\ pool_f + \mu' x_f + \varepsilon_f$$

The dependent variable is the change in an indicator variable that equals 1 if a cotton mill laid off workers (Panel A), reduced electricity consumption (Panel B), reduced output (Panel C) in a given year, and 0 otherwise (average after 1933 minus average prior to 1933). The variable $Silver\ pool$ is the inverse distance-weighted average silver reserves around firm $f$ (columns (1)-(2)), the inverse distance-weighted average of the silver amount in excess to compulsory reserves, in columns (3)-(4) ($Excess\ silver\ pool$), or the inverse distance-weighted average of the indicator variable taking the value of 1 if a given bank has silver-to-notes ratio above 60%, and 0 otherwise, in columns (5)-(6) ($Excess\ reserves\ pool$). Specifications (2), (4) and (6) include firm nationality fixed effects. Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around 1933. All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses.

| **A. Dep. variable: $\Delta Layoff$** | | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver pool | -1.186*** | -1.178*** | | | | |
| | (0.257) | (0.259) | | | | |
| Excess silver pool | | | -0.879*** | -0.872*** | | |
| | | | (0.185) | (0.184) | | |
| Excess reserves pool | | | | | -4.159* | -4.035* |
| | | | | | (2.058) | (2.115) |
| Intercept | 7.890*** | | 4.004*** | | 1.950* | |
| | (1.756) | | (0.888) | | (0.963) | |
| Nationality f.e. | N | Y | N | Y | N | Y |
| N | 38 | 38 | 38 | 38 | 38 | 38 |
| $R^2$ | 0.11 | 0.12 | 0.12 | 0.12 | 0.05 | 0.06 |

| B. Dep. variable: $\Delta Power\ consumption$ | | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver pool | -6.212** | -6.947** | | | | |
| | (2.675) | (2.584) | | | | |
| Excess silver pool | | | -3.752** | -3.995** | | |
| | | | (1.595) | (1.546) | | |
| Excess reserves pool | | | | | -13.327** | -15.072*** |
| | | | | | (5.284) | (5.022) |
| Intercept | 40.845** | | 16.683** | | 6.056** | |
| | (17.540) | | (7.049) | | (0.975) | |
| Nationality f.e. | N | Y | N | Y | N | Y |
| N | 29 | 29 | 29 | 29 | 29 | 29 |
| $R^2$ | 0.19 | 0.30 | 0.16 | 0.24 | 0.15 | 0.25 |

| C. Dep. variable: $\Delta Cloth\ output$ | | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver pool | -0.524* | -0.524* | | | | |
| | (0.259) | (0.256) | | | | |
| Excess silver pool | | | -0.406** | -0.400** | | |
| | | | (0.189) | (0.182) | | |
| Excess reserves pool | | | | | -2.739* | -2.402 |
| | | | | | (1.572) | (1.434) |
| Intercept | 3.594** | | 1.953** | | 1.381* | |
| | (1.736) | | (0.872) | | (0.740) | |
| Nationality f.e. | N | Y | N | Y | N | Y |
| N | 37 | 37 | 37 | 37 | 37 | 37 |
| $R^2$ | 0.04 | 0.14 | 0.05 | 0.14 | 0.05 | 0.13 |

# Appendices

# Appendix 1.A.

## Interpreting the Effects of the Silver Purchase Program: Framework

*A.1 Economic historiography of the Silver Purchase program*

The argument that the Silver Purchase Program had negative effects on the Chinese economy can be traced back to contemporary commentators such as Lin (1936, pp. 5-77) and Leavens (1939, pp. 293-312). Friedman and Schwartz (1963) and Friedman (1992) argue that the Silver Purchase program had a devastating impact. As silver was at the basis of the Chinese monetary standard, an outflow of silver corresponded to both a contraction in the money supply and an appreciation of the Chinese dollar vis-à-vis major foreign currencies. The decline in money supply produced a sharp reduction of imports, domestic consumption, and investment. At the same time, rising silver prices corresponded to an appreciation of the Chinese dollar, with detrimental effects on exports. Compared to 1929, the export value of China's major goods such as silk and tea was down by 65% in 1935 (Yu (1937, pp. 224-225)).

Rawski (1989 and 1993) disputes Friedman's findings and asserts that the silver outflow had a negligible impact on the Chinese money supply. Moreover, the Chinese economy already experienced deflation from 1931 and in 1935 the price level rose.

Brandt and Sargent (1989) recognize that the Silver Purchase program led to an increase of silver prices and an outflow of silver from China. However, they argue that the program had mainly an effect on relative prices, but not on the real economy. With higher silver prices, Chinese banks could back up the same, or an even larger, amount of paper money with any given amount of silver. Brandt and Sargent (1989) further argue that Chinese banks exploited the arbitrage opportunity offered by higher silver prices and sold part of the silver abroad. As a result, they replaced part of their silver reserves with Republic of China Treasury bonds.

Brandt and Sargent's (1989) argument rests on two assumptions. First, Chinese Treasury bonds were "as good as silver" to back up the currency (i.e. the perceived risk of sovereign default was very low). This has become known as the "real bills" doctrine (e.g. Sargent and Wallace (1982)). Second, prices in China were flexible enough to insulate the real economy from any adverse effects of the outflow of silver and deflation. Consistent with this hypothesis, they show that M1 declines as a result of the outflow of Silver, but M2 remains constant or even increases during the 1930s. They also present macroeconomic evidence showing only a mild decline in GPD and other macroeconomic aggregates.

Burdekin (2008) presents evidence supporting the Friedman and Schwartz (1963) and Friedman (1992) line of argument. He highlights how geographical differences in China are important in explaining the unfolding of the Silver Purchase shock. Shanghai, for instance, received large quantities of silver from the interior and, as a result, it was partially insulated from the shock until 1934. Internal areas experienced a sharp outflow of

silver already starting in 1933, leading banks into financial distress and sharply reducing the price level. He also presents macroeconomic time series evidence that links the silver purchase program to deflation, exchange rate appreciation, and bank distress in China.

The evidence we present in the text is in line with this interpretation. In addition, we find that, at the macroeconomic level, credit sharply contracts in China after 1933, based on a variety of aggregate measures. Figure A.1 summarizes this evidence, showing that credit-to-GDP drops by nearly 15% between 1933 and 1935 (panel A), while aggregate credit-to-deposits and credit-to-total bank assets ratios drop by about 10% (panel B). Moreover, the credit contraction appears circumscribed to the Republic of China. There is no evidence, for instance, of a comparable credit contraction in neighboring Hong Kong. Based on data available for HSBC, at the time the largest credit institution in Hong Kong, it appears that credit provision, in fact, increased there between 1933-35 (panel C).

At the end of the day, whether or not the Silver Purchase program had an impact on the Chinese real economy remains an empirical question. We examine in our tests a specific consequence of it: the silver outflow's effect on credit provision and social stability. To the extent that silver was used to back the currency, an outflow of silver would drain banks of the necessary resources needed to support lending, thus leading to a credit crunch. Below we present a simple model, nesting the Friedman and Schwarz (1963) and Brandt and Sargent (1989) interpretations, which can be directly linked to our empirical analysis.

### *A.2 Impact of the Silver Purchase program on lending*

We consider a simple model illustrating the impact of the Silver Purchase program on the Chinese credit market, encompassing both the Friedman and Schwartz (1963) and Friedman (1992) interpretation and the alternative interpretation of Brandt and Sargent (1989).

In 1930s China, modern banks serve two key roles: they generate the money supply, by issuing bank notes, and credit, by making loans. Consider for simplicity a bank financed entirely by bank notes, in an amount $N$. Under a pure silver standard, banknotes should be 100% backed by an equivalent amount of silver. In practice, the law allows the bank to issue a volume of notes larger than its silver reserves $S$, as long as the silver reserves ratio $\sigma = S/N$ is equal to $\bar{\sigma} = 60\%$ or greater. The remaining $(1-\sigma)N$ is "collateralized" by Treasury bonds with a total face value $B$. Banks convert bonds into banknotes, valuing bonds at their face value, which we normalize to 1.[37]

[37] This assumption is based on historical evidence. Cheng (2003, p. 160) reports that banks converted Treasury bonds into banknotes valuing the bonds at their face value (even when they traded below par), but usually bonds were recorded on the assets of the banks at their market values. Between 1927 and 1928, the Nationalist government also introduced legislation intended to regulate the banking sector. In particular, the registration of new financial institutions had to follow new and detailed procedures, and banks were subject to capital requirements (Chen (2003), p. 90-91). The government also created a banking supervisory authority, called Bureau of Financial Supervision, with the intention of investigating the business of all the

Suppose now that Treasury bonds can be purchased on the market at a price $p_B \leq 1$, so that the market value of the bank's reserves is $S + p_B B$.[38] Having issued bank notes for a value $N$, the bank has a surplus $N - S - p_B B$ that can generate loans $L$; simple manipulations show that $L = (1 - p_B)\frac{1-\sigma}{\sigma}S$. Thus, if government bonds trade at their face value and $p_B = 1$ (or if a 100% silver reserve is mandated), in this simplified setting the bank only generates the money supply, but not credit. This situation corresponds to the interpretation of Brandt and Sargent (1989), where shocks to the silver supply will not have any impact on lending.

The ability to make loans derives from the "arbitrage" between the face and market value of the non-silver collateral. However, to the extent that the bank's silver reserves do not cover the full amount of bank notes in circulation $N$, the bank is exposed to a "run." If all holders of banknotes want to convert them into silver, the bank has a silver shortage $(1 - p_B)(1 - \sigma)N$. Let $\pi$ denote the probability of a run, and assume that the bank faces a quadratic cost $\frac{\gamma}{2}[(1 - p_B)(1 - \sigma)N]^2$ in the event of a run. This can be interpreted either as the bank requiring an equity injection to overcome the shortfall or as an early liquidation of the outstanding loans.

Summing up, the bank finances with an amount of notes $N$ its investment in reserves $S + p_B B$ plus loans $L$, facing the expected cost of a run equal to $\pi\frac{\gamma}{2}[(1 - p_B)(1 - \sigma)N]^2$. Denoting the marginal return on loans by $r_L$, the bank's profit function is thus: $\Pi(\sigma, S) = r_L L + S + p_B B - N - \pi\frac{\gamma}{2}[(1 - p_B)(1 - \sigma)N]^2$. Expressing all the relevant quantities in terms of $S$ and the reserve ratio $\sigma$, the bank's optimization problem is:

$$\max_{\sigma}(r_L - 1)(1 - p_B)\frac{1-\sigma}{\sigma}S - \pi\frac{\gamma}{2}\left[(1 - p_B)\frac{1-\sigma}{\sigma}S\right]^2. \qquad \text{(A.1)}$$

The bank's optimal reserve and lending policy is thus determined, subject to the reserves constraint $\sigma \geq \bar{\sigma} = 60\%$.

Assume, as in Khwaja and Mian (2008), a linear loan demand $r_L = \bar{r} - \alpha_L L$, which the bank takes as given (i.e. a competitive credit market). Under this assumption, solving (A.1) the bank holds a fraction of silver reserves:

$$\hat{\sigma} = \max\left\{\bar{\sigma}, \left[1 + \frac{\bar{r}-1}{S(1-p_B)(\alpha_L+\pi\gamma)}\right]^{-1}\right\}. \qquad \text{(A.2)}$$

If the reserves constraint is not binding, it will make an amount of loans equal to:

$$\hat{L} = (\alpha_L + \pi\gamma)^{-1}(\bar{r} - 1). \qquad \text{(A.3)}$$

If the reserves constraint is binding, $\hat{\sigma} = \bar{\sigma}$ and the bank makes an amount of loans:

$$\hat{L}' = (1 - p_B)\frac{1-\bar{\sigma}}{\bar{\sigma}}S. \qquad \text{(A.3')}$$

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Chinese banks. However, the authority had neither the power nor the capacity to carry out its duties. While the four largest banks played a relevant role in placing treasury bonds on the market, there was no modern central bank entrusted to set interest rates or regulate the money supply.

[38] We are using silver as the numeraire, so that $p_B$ is in fact the market price of government bonds relative to the market price of silver.

Thus, if collateral other than silver is "as good as silver" ($p_B = 1$), as assumed by Brandt and Sargent (1989), shocks to the silver supply cannot affect the credit market – they just affect the money supply. In contrast, if $p_B < 1$, shocks to the silver supply will be reflected in the credit supply as soon as the reserves constraint becomes binding.

These observations allow us to understand the impact of the Silver Purchase. Suppose that the above model is played over two dates $t = 0, 1$ (before and after the Silver Purchase). At each date the bank can lend on the credit market, and firms demand loans; assume that, prior to the Silver Purchase, the bank's reserves constraint is not binding, i.e. $S > \bar{\sigma}N$, and the credit market equilibrium is described by (A.3).

At $t = 1$, the bank's silver reserves are hit by a shock bringing them down to $S - \Delta S$, so that the bank might need to adjust its lending decisions.[39] The shock to reserves captures the attempt by banknotes holders to convert them into silver, to profit from the Silver Purchase. It is apparent from the above expressions that the silver shock will affect equilibrium loans only if the bank becomes constrained; this requires $(S - \Delta S) < \bar{\sigma}(N - \Delta S)$, i.e. $\Delta S > \frac{\bar{r}-1}{(1-\bar{\sigma})(1-p_B)(\alpha_L+\pi\gamma)}$. In that case, the change in equilibrium lending becomes:

$$\Delta\hat{L} = (1 - p_B)\frac{1-\bar{\sigma}}{\bar{\sigma}}(S - \Delta S) - \frac{\bar{r}-1}{\alpha_L+\pi\gamma}. \quad \text{(A.4)}$$

Thus, a lower pre-1933 level of silver reserves $S$ is associated with a larger reduction in lending $\Delta\hat{L}$.

To take the above expression to the data, we follow Khwaja and Mian (2008) and Schnabl (2012) and we assume that, while each bank $i$ lends to only one firm $j$, any given firm can borrow from multiple banks. Furthermore, we allow for a shock to loan demand bringing it to $\bar{r}_j + \Delta\bar{r}_j - \alpha_L L$, so that the above expression becomes:

$$\Delta L_{ij} = (1 - p_B)\frac{1-\bar{\sigma}}{\bar{\sigma}}(S_i - \Delta S) - \frac{\bar{r}_j+\Delta\bar{r}_j-1}{\alpha_L+\pi\gamma}. \quad \text{(A.5)}$$

Rewriting the above expression more compactly, we thus estimate, in section IV:

$$\Delta L_{ij} = \delta S_i + \eta_j + \varepsilon_{ij}, \quad \text{(A.6)}$$

i.e. regress changes in loans from bank $i$ to firm $j$ around the Silver Purchase program on the bank's pre-shock silver holdings $S_i$, including borrowing firm fixed effects $\eta_j$. As we discuss in the text, the above is equivalent to a differences-in-differences setup, where the bank's pre-Silver Purchase program silver reserves are the treatment.

[39] Equivalently, one could rewrite the model having the bank purchase additional silver on the market in order to make new loans, so that a rise in the market price of silver restricts the lending supply. This would have identical predictions on the impact of the Silver Purchase program on banks with high and low ex ante silver reserves (in this case, of course, the numeraire would necessarily be a variable other than silver, e.g. government bonds).


**Figure A.1 Silver Flows, Foreign Banks in Shanghai (1929-1935)**
The graph report the net percentage change (flow) in silver holdings of non-Chinese banks in Shanghai between 1929 and 1935. It indicates that silver holdings decline after the enactment of the Silver Purchase Program in 1933. Data on foreign banks' silver holdings are retrieved from Tamagna (1949, p. 104).

# Appendix 1.B.

## Variable Definitions

| Variable | Definition |
|---|---|
| *Total loans* | The total annual amount of outstanding loans of a bank in a given year (expressed in units of Ch$ 10 millions, in the tests in Table 3 and 8). |
| *Loan amount* | The loan amount granted by a bank to a firm (expressed in units of Ch$ 10 thousands in Tables 4, 7, and 9). |
| *Number of unrest episodes* | Number of labor unrest episodes that occur at a given plant in a given year. |
| *Duration* | Number of days of labor unrest that a given plant experiences in a given year. |
| *Communist activities* | Number of times a given firm is mentioned in the Shanghai Municipal Police files in relation to either (1) The arrest of at least one of his employees for allegedly communist activities or (2) Being targeted for penetration by a communist cell in Shanghai. |
| *Silver* | The silver reserves of a bank measured in 1931, or the earliest available date prior to 1933 (in our sample, never later than 1932). |
| *Excess silver* | The silver reserves in excess of the mandatory requirement of 60% of outstanding banknotes issued by a bank in 1931, or the earliest available date prior to 1933 (in our sample, never later than 1932). |
| *Excess reserves (Y/N)* | An indicator variable that takes the value of 1 if a bank has silver-to-notes ratio in excess of the 60% threshold in 1931, or the earliest available date prior to 1933 (in our sample, never later than 1932), and 0 otherwise. In order to attenuate the potential effect of noise in the reserves data, we apply a 1% margin (i.e. the indicator equals 1 for reserves of at least 60.06%). |
| *Traded sector* | An indicator variable that takes the value of 1 if a firm belongs to one of the following sectors: Chemicals, Textile, Clothing, Food, Concrete & Glass, Leather, Machines & Metal, Paper & Printing, Transportation tools manufacture, Wood, Farming, and Other manufacture (Mano and Castillo (2015)). |
| *Silver pool* | The inverse distance-weighted average silver reserves around a given firm's plant. For each firm plant $f$ in the sample, it is computed as: $$Silver\ pool_f = \sum_b \frac{Silver_b / d(f,b)}{\sum_b 1/d(f,b)}$$ where $Silver_b$ denotes the silver reserves of bank $b$ as of 1931, or 1932 if not available, and $d(f,b)$ the distance between plant $f$ and bank $b$ (measured in km). |

| | |
|---|---|
| *Excess silver pool* | The inverse distance-weighted average excess silver reserves around a given firm's plant. For each firm plant $f$ in the sample, it is computed as: $$Excess\ silver\ pool_f = \sum_b \frac{Excess\ silver_b / d(f,b)}{\sum_b 1/d(f,b)}$$ where $Excess\ silver_b$ denotes the excess silver reserves of bank $b$ as of 1931, or 1932 if not available, and $d(f,b)$ the distance between plant $f$ and bank $b$ (measured in km). |
| *Excess reserves (Y/N) pool* | The inverse distance-weighted average of an indicator variable taking the value of 1 if a given bank has silver-to-notes ratio above 60%, and 0 otherwise. For each firm plant $f$ in the sample, it is computed as: $$Excess\ reserves\ (Y/N)\ pool_f = \sum_b \frac{Excess\ reserves\ (Y/N)_b / d(f,b)}{\sum_b 1/d(f,b)}$$ where $Excess\ reserves\ (Y/N)_b$ denotes the dummy variable taking the value of 1 if silver-to-notes ratio of bank $b$ as of 1931, or 1932 if not available is above 60%, and $d(f,b)$ the distance between plant $f$ and bank $b$ (measured in km). |
| *Copper pool (Loans)* | The inverse distance-weighted average copper capacity of the nearest 3 copper mines around the headquarters of a given bank. |
| *(w.a.) Copper pool (Labor unrest and Communist activities)* | The inverse distance-weighted average copper capacity of the nearest 3 copper mines to a given firm's closest bank. |
| *Bank size* | Total amount of bank's equity. |
| *Equity ratio* | Bank equity divided by total assets (expressed in percentage points). |
| *Cash ratio* | Bank cash holdings divided by total assets (expressed in percentage points). |
| *ROE* | Bank net income divided by bank equity (expressed in percentage points). |
| *No ret. earnings* | An indicator variable that takes the value of 1 if a given bank has 0 retained earnings in a given year, and 0 otherwise. |
| *No notes* | An indicator variable taking the value of 1 if a given bank has not issued any bank notes, and 0 otherwise. |
| *Relation* | An indicator variable taking the value of 1 if a given bank-firm pair has relation, and 0 otherwise. |
| *Distance* | Distance, measured in kilometers, between a bank branch and a plant. |
| *Operation* | An indicator variable that takes the value of 1 if a given cotton mill lays off any employees in a given year (Table 10.A), reduces electricity consumption (Table 10.B), or cuts down production (Table 10.C), and 0 otherwise. |

# Appendix 1.C.

## Additional Tests

This appendix contains additional results that are omitted from the main text of the paper for brevity.

Tables C.1 and C.2 report estimates corresponding to the models presented in Tables 7 and 9.B respectively, with the log-duration of labor unrest episodes as the dependent variable.

Table C.3 reports the first-stage estimates associated with the IV estimates reported in Table 9.A and 9.B.

Tables C.4, C.5, C.6, and C.7 report the estimates of panel regressions with fixed effects, corresponding to the models estimated in Tables 3, 4, 5.A, and 6. In this case, the data are not collapsed and time-averaged before and after 1933, but the standard errors are clustered.

Tables C.8 and C.9 report the estimates of models identical to the ones reported in Tables 5.A and 6, excluding from the sample firms located in the Zhabei (闸北) district in Shanghai, which may be affected by Japanese interests. The estimates are very close to the ones in Tables 5.A and 6, suggesting that Japanese interference is unlikely related to our results.

**Table C.1 Traded versus Non-traded Sectors: Labor Relation around 1933 – Unrest Episode Duration**

The table reports the estimates of regressions following specifications (4)-(6) of Table 5.A. All specifications include the additional variables $Traded\ sector$ (an indicator equals to 1 if sectors produce tradable products) and $Traded\ sector \times Silver\ pool$. The regressions in all columns include city district, industry and firm nationality fixed effects. Following Bertrand, Duflo, and Mullainathan (2004), all regressions are estimated on changes around 1933. All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses.

| **Dependent var.** | $\Delta\log(1 + Duration)$ | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Silver | -0.273 | | |
| | (0.557) | | |
| Silver × Traded sector | -0.394 | | |
| | (0.608) | | |
| Excess silver | | -0.272 | |
| | | (0.522) | |
| Excess silver × Traded sector | | -0.379 | |
| | | (0.566) | |
| Excess reserves (Y/N) | | | -0.598 |
| | | | (1.457) |
| Excess reserves (Y/N) × Traded sector | | | -0.874 |
| | | | (1.593) |
| District f.e. | Y | Y | Y |
| Industry f.e. | Y | Y | Y |
| Nationality f.e. | Y | Y | Y |
| N | 1,500 | 1,500 | 1,500 |
| $R^2$ | 0.08 | 0.08 | 0.08 |

**Table C.2 Impact of the Liquidity Shock on Labor Relations – IV Estimation, 2nd Stage – Unrest Episode Duration**

The table reports the estimates of:

$$\Delta Labor\ unrest_f = \delta + \gamma Silver\ pool_f + \mu' x_f + \varepsilon_f$$

following the same specifications as in columns (4)-(6) in Table 5.A. $Silver\ pool$ is instrumented by copper availability $Copper\ pool$, defined as the inverse distance-weighted average of the natural logarithm of copper capacity in the nearest 3 copper mines to a firm's closest bank $b$. All columns include city district, industry, and firm nationality fixed effects. Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around 1933. All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses.

| **Dep. variable: $\Delta\log(1 + Duration)$ (2nd stage)** | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Silver pool | -2.540*** | | |
| | (1.013) | | |
| Excess silver pool | | -2.261** | |
| | | (0.908) | |
| Excess reserves pool | | | -7.756** |
| | | | (3.068) |
| District f.e. | Y | Y | Y |
| Industry f.e. | Y | Y | Y |
| Nationality f.e. | Y | Y | Y |
| N | 1,500 | 1,500 | 1,500 |
| First stage F-stat | 129.75 | 124.33 | 115.40 |

**Table C.3 Credit and Social Unrest around 1933 – IV Estimation, 1st Stage**
Panel A reports the first-stage estimates of Table 9.A. The first stage is specified as:

$$Silver_{b,1931} = \eta_f + \rho Copper\ pool_b + \mu' \Delta x_{bf} + \nu_{bf}$$

All specifications include a full set of borrowing firm fixed effects. The standard errors, reported in parentheses, are clustered at the bank level. Panel B reports the first stage estimates of Table 8.B and Table C.1. The first stage is specified as:

$$Silver\ pool_f = \eta + \rho\ (w.a.)\ Copper\ pool_f + \mu' x_f + \nu_f$$

All specifications include city district, industry and firm nationality fixed effects. Specifications (1)-(3) correspond to specifications (1)-(3) of Table 9.B; specifications (4)-(6) correspond to specifications (4)-(6) of Table 9.B. The Huber-White robust standard errors are reported in parentheses. All variables are time-averaged before and after 1933. All variables are defined in detail in Appendix B.

**A. Dep. variable: *Silver reserves* (1st stage)**

| | Silver | Excess silver | Excess reserves (Y/N) |
|---|---|---|---|
| | (1) | (2) | (3) |
| Copper pool | -4.872*** | -5.417* | -1.884*** |
| | (1.526) | (2.750) | (0.448) |
| Control var. | Y | Y | Y |
| Firm f.e. | Y | Y | Y |
| N | 229 | 229 | 229 |
| $R^2$ | 0.71 | 0.38 | 0.69 |

**B. Dep. variable: *Silver pool* (1st stage)**

| | Silver pool | Excess silver pool | Excess reserves pool | Silver pool | Excess silver pool | Excess reserves pool |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| (w.a.) Copper pool | -0.142*** | -0.161*** | -0.045*** | -0.172*** | -0.193*** | -0.056*** |
| | (0.012) | (0.014) | (0.004) | (0.015) | (0.017) | (0.005) |
| District f.e. | Y | Y | Y | Y | Y | Y |
| Industry f.e. | Y | Y | Y | Y | Y | Y |
| Nationality f.e. | Y | Y | Y | Y | Y | Y |
| N | 1,743 | 1,743 | 1,743 | 1,500 | 1,500 | 1,500 |
| $R^2$ | 0.89 | 0.89 | 0.92 | 0.41 | 0.38 | 0.45 |

**Table C.4 Silver Reserves and Credit around 1933 – Bank-Level (No Collapsing)**
The table reports replication of estimation in Table 3, without collapsing and time-averaging the data before and after 1933. All variables are defined in detail in Appendix B. The standard errors, reported in parentheses, are clustered by bank.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Silver × Post | 0.119*** | 0.140*** | 0.136*** | | | | |
| | (0.039) | (0.028) | (0.029) | | | | |
| Excess silver × Post | | | | 0.391*** | 0.382*** | | |
| | | | | (0.079) | (0.080) | | |
| Excess reserves (Y/N) × Post | | | | | | 0.131*** | 0.134*** |
| | | | | | | (0.036) | (0.037) |
| Control var. | N | Y | Y | Y | Y | Y | Y |
| Bank f.e. | Y | Y | Y | Y | Y | Y | Y |
| Year f.e. | Y | Y | Y | Y | Y | Y | Y |
| No notes bank × Year f.e. | N | N | Y | N | Y | N | Y |
| N | 214 | 211 | 365 | 211 | 365 | 211 | 365 |
| $R^2$ | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 |

**Table C.5 Silver Reserves and Credit around 1933 – Loan-Level (No Collapsing)**
The table reports replication of estimation in Table 4, without collapsing and time-averaging the data before and after 1933. All columns include bank-plant pair, year and firm-year fixed effects. All variables are defined in detail in Appendix B. The standard errors, reported in parentheses, are clustered by bank.

| | (1) | (2) | (3) |
|---|---|---|---|
| Silver × Post | 0.159*** | | |
| | (0.047) | | |
| Excess silver × Post | | 0.116*** | |
| | | (0.029) | |
| Excess reserves (Y/N) × Post | | | 0.351*** |
| | | | (0.140) |
| Control var. | Y | Y | Y |
| Bank-plant pair f.e. | Y | Y | Y |
| Year f.e. | Y | Y | Y |
| Firm × Year f.e. | Y | Y | Y |
| N | 1,077 | 1,077 | 1,077 |
| $R^2$ | 0.84 | 0.84 | 0.84 |

**Table C.6 Impact of the Liquidity Shock on Social Unrest – Labor Relations (No Collapsing)**
The table reports the replication of estimation in Table 5.A, without collapsing and time-averaging the data before and after 1933. All columns include plant, year, district-year, firm industry-year, and firm nationality-year fixed effects. All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses.

| **Dep. variable:** | $\Delta\log(1 + Number\ of\ labor\ unrest\ episodes)$ | | | $\Delta\log(1 + Duration)$ | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver pool × Post | -0.243*** | | | -0.611** | | |
| | (0.065) | | | (0.244) | | |
| Excess silver pool × Post | | -0.227*** | | | -0.602*** | |
| | | (0.056) | | | (0.207) | |
| Excess reserves pool × Post | | | -0.673*** | | | -1.326* |
| | | | (0.208) | | | (0.783) |
| Plant f.e. | Y | Y | Y | Y | Y | Y |
| Year f.e. | Y | Y | Y | Y | Y | Y |
| District × Year f.e. | Y | Y | Y | Y | Y | Y |
| Industry × Year f.e. | Y | Y | Y | Y | Y | Y |
| Nationality × Year f.e. | Y | Y | Y | Y | Y | Y |
| N | 8,632 | 8,632 | 8,632 | 7,500 | 7,500 | 7,500 |
| $R^2$ | 0.49 | 0.50 | 0.49 | 0.43 | 0.43 | 0.43 |

**Table C.7 Impact of the Liquidity Shock on Social Unrest – Communist Activities (No Collapsing)**

The table reports the replication of estimation in Table 6, without collapsing and time-averaging the data before and after 1933. Columns (1), (4), and (7) include plant and year fixed effects. Columns (2), (5), and (8) include plant, year, firm industry-year, and firm nationality-year fixed effects. Columns (3), (6), and (9) include plant, year, district-year, firm industry-year, and firm nationality-year fixed effects. All variables are defined in detail in Appendix B. The Huber-White robust standard errors are reported in parentheses.

| **Dep. Variable: $\Delta$*Communist activities*** | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Silver pool × Post | -0.085 | | | -0.083 | | | -0.167 | | |
| | (0.049) | | | (0.049) | | | (0.132) | | |
| Excess silver pool × Post | | -0.058 | | | -0.058 | | | -0.109 | |
| | | (0.032) | | | (0.028) | | | (0.105) | |
| Excess reserves pool × Post | | | -0.056 | | | -0.062 | | | -0.061 |
| | | | (0.043) | | | (0.034) | | | (0.144) |
| Plant f.e. | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Year f.e. | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| District × Year f.e. | N | N | Y | N | N | Y | N | N | Y |
| Industry × Year f.e. | N | Y | Y | N | Y | Y | N | Y | Y |
| Nationality × Year f.e. | N | Y | Y | N | Y | Y | N | Y | Y |
| N | 7,500 | 7,500 | 7,500 | 7,500 | 7,500 | 7,500 | 7,500 | 7,500 | 7,500 |
| $R^2$ | 0.42 | 0.51 | 0.51 | 0.42 | 0.51 | 0.51 | 0.42 | 0.51 | 0.51 |

**Table C.8 Impact of the Liquidity Shock on Social Unrest – Labor Unrest (Excluding Zhabei District)**
The table reproduces the estimates of Table 5.A, excluding firms located in the Zhabei (闸北) district in Shanghai.

| **Dep. variable:** | $\Delta\log(1 + Number\ of\ labor\ unrest\ episodes)$ | | | $\Delta\log(1 + Duration)$ | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Silver pool | -0.242*** | | | -0.604** | | |
| | (0.071) | | | (0.268) | | |
| Excess silver pool | | -0.224*** | | | -0.588*** | |
| | | (0.061) | | | (0.226) | |
| Excess reserves pool | | | -0.627*** | | | -1.138 |
| | | | (0.223) | | | (0.842) |
| District f.e. | Y | Y | Y | Y | Y | Y |
| Industry f.e. | Y | Y | Y | Y | Y | Y |
| Nationality f.e. | Y | Y | Y | Y | Y | Y |
| N | 1,497 | 1,497 | 1,497 | 1,254 | 1,254 | 1,254 |
| $R^2$ | 0.12 | 0.12 | 0.12 | 0.08 | 0.08 | 0.08 |

**Table C.9 Impact of the Liquidity Shock on Social Unrest – Communist Activities (Excluding Zhabei District)**
The table reproduces the estimates of Table 6, excluding firms located in the Zhabei (闸北) district in Shanghai.

| **Dep. Variable: $\Delta\log(1 + Number\ of\ communist\ activities)$** | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Silver pool | -0.085** | -0.096** | -0.103** | | | | | | |
| | (0.033) | (0.039) | (0.047) | | | | | | |
| Excess silver pool | | | | -0.076** | -0.084** | -0.099** | | | |
| | | | | (0.030) | (0.034) | (0.039) | | | |
| Excess reserves pool | | | | | | | -0.174* | -0.193 | -0.182 |
| | | | | | | | (0.104) | (0.119) | (0.151) |
| Intercept | 0.547** | | | 0.329** | | | 0.071 | | |
| | (0.217) | | | (0.131) | | | (0.045) | | |
| District f.e. | N | N | Y | N | N | Y | N | N | Y |
| Industry f.e. | N | Y | Y | N | Y | Y | N | Y | Y |
| Nationality f.e. | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 1,254 | 1,254 | 1,254 | 1,254 | 1,254 | 1,254 | 1,254 | 1,254 | 1,254 |
| $R^2$ | 0.22 | 0.27 | 0.27 | 0.23 | 0.27 | 0.27 | 0.22 | 0.27 | 0.27 |

# Appendix 1.D.

## Data on Loans, Labor Unrest Episodes, and Underground Communist Activities

This Appendix presents excerpts from our primary sources of data:

- Loan contracts (Figure D.1);
- Survey data on labor unrest episodes (Figure D.2);
- SMP files on underground communist activities in Shanghai (Figure D.3).

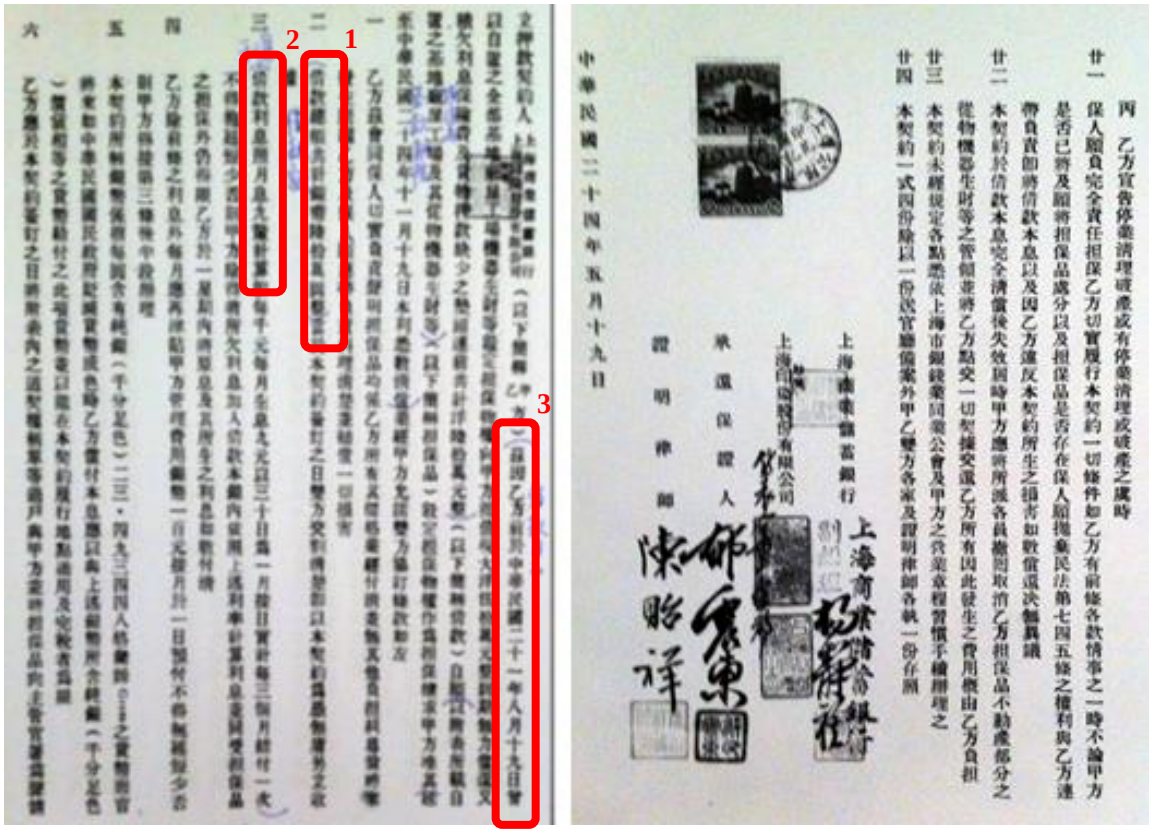
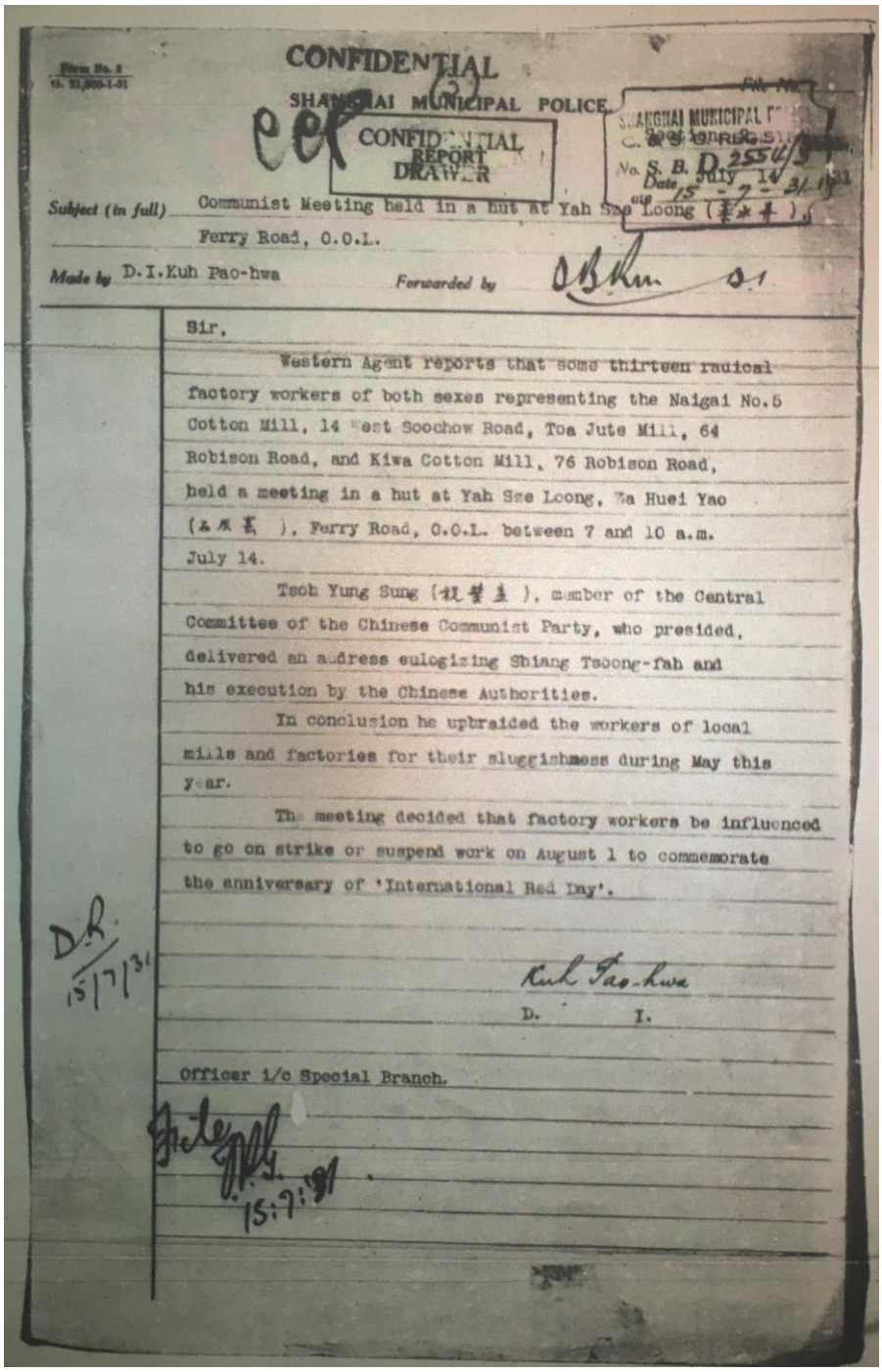



**Figure D.1 Sample Loan Contract – Excerpt**
The figure shows the first and last pages of one of the loan contracts in the sample. The loan is made by Shanghai Commercial and Savings Bank to Shanghai Print and Dye Co. (上海印染股份有限公司). The page on the left reports the loan amount (600,000 Chinese dollars, red circle 1.), the interest rate (0.9% on a monthly basis, red circle 2.), and the date of the contract (19 August 1932, red circle 3.). The page on the right reports the signatures of the loan officer, a firm representative, a guarantor, and the notary on the loan.

**Figure D.2 Shanghai Survey of Labor Unrest Episodes – Excerpt**

The figure shows an excerpt from the survey *Industrial Disputes in Shanghai since 1928* (近五年来上海之劳资纠纷), conducted by the Bureau of Social Affairs of the city government of greater Shanghai between 1931 and 1932, and used in the analysis. Each row in the table refers to an individual labor unrest episode. The table's columns report the episode's date (col. 1), the id of the case (col. 2), the motivation for the episode (col. 3), the industry of the affected company (col. 4) and its nationality (col. 5), the number of factories involved in the episode (col. 6), the number of workers involved in the episode (col. 7), the duration of the episode (col. 8), the office handling the episode (col. 9), and its final outcome (col. 10).



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SHANGHAI MUNICIPAL POLICE C.I.D. SPECIAL BRANCH

No. S. B. D. 2554/31

Date July 14

Subject (in full) Communist Meeting held in a hut at Yah Sze Loong (亞斯弄), Ferry Road, O.O.L.

Made by D.I.Kuh Pao-hwa Forwarded by

Sir,

Western Agent reports that some thirteen radical factory workers of both sexes representing the Naigai No.5 Cotton Mill, 14 West Soochow Road, Toa Jute Mill, 64 Robison Road, and Kiwa Cotton Mill, 76 Robison Road, held a meeting in a hut at Yah Sze Loong, Ta Huei Yao (大會堯), Ferry Road, O.O.L. between 7 and 10 a.m. July 14.

Tsoh Yung Sung (祖榮生), member of the Central Committee of the Chinese Communist Party, who presided, delivered an address eulogizing Shiang Tsoong-fah and his execution by the Chinese Authorities.

In conclusion he upbraided the workers of local mills and factories for their sluggishness during May this year.

The meeting decided that factory workers be influenced to go on strike or suspend work on August 1 to commemorate the anniversary of 'International Red Day'.

Kuh Pao-hwa

D. I.

Officer i/c Special Branch.

**Figure D.3 Shanghai Municipal Police Records – Excerpt**

The figure shows an excerpt from the Shanghai Municipal Police records used in the analysis, reporting the findings of an agent infiltrated in the underground Communist Party. The excerpt, dated 15 July 1931, summarizes the meeting of a party cell. At the meeting, cell members were instructed to "go on strike or suspend work on August 1 to commemorate the anniversary of 'International Red Day.'"

# Chapter 2

## Social Stability and Resource Allocation within Business Groups

**Abstract**

Using datasets on transactions within business groups and social sentiment in China, I show that state-owned enterprises (SOEs) use internal funds to address social unrest, complying with the government's political goals. I use plausibly unexpected shocks to regional stability to analyze the response of SOEs to these events. I find that the government, as the controlling shareholder, adopts a carrot-and-stick approach. The government offers a "carrot" by injecting funds into SOEs located in the affected areas, which are then used to generate benefits to the public, such as larger labor payments and additional capital expenditures. However, if there are severe political conflicts that threaten its authority, the government applies a "stick" by withdrawing resources. The SOEs channel seems effective because local sentiment recovers around shocks when SOEs offer more benefits. Additional tests show that the SOEs channel is significant in economic magnitude compared to fiscal redistribution. As a result of the transfer, SOEs lose value after shocks, but firms in the region improve their performance later. This paper provides new evidence on how the intra-group allocation of resources incorporates political objectives, and has socioeconomic impact.

JEL: G32, G34, D74, P16.

Keywords: business groups, internal capital market, political economy, social stability.

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## 2.1. Introduction

The literature on the economic merits of state and private ownership provides ample evidence that SOEs are less efficient than their private counterparts.[40] These differences in efficiency have been attributed to agency problems caused by the weak incentives offered to SOE managers and by poor monitoring, as well as to politicians' rent-seeking at the expense of minority shareholders (Shleifer and Vishny (1994, 1998), Shleifer (1998)). Successive waves of privatization since the 1980s have led to a marked reduction in state ownership, yet, in the post-privatization era, SOEs continue to be among the largest corporations in the world and account for a substantial share of the economy in many countries. Furthermore, recent years have witnessed a resurgence of state ownership, especially in emerging markets (Hsu, Liang, and Matos (2017); Musacchio and Lazzarini (2014); Musacchio, Lazzarini, and Aguilera (2015)).

What functions does state ownership carry to compensate for its economic inefficiency for government shareholders? Hart and Zingales (2017) imply that such functions lie in the embedded incentives for SOEs to address governments' non-economic goals, and in the government's willingness to trade short-run profits for achieving such goals. Their model shows that in the presence of shareholders who care about social issues, the function of the firms will be to maximize shareholders' welfare by internalizing non-economic goals, rather than just wealth. Such shareholders are willing to accept lower returns when a firm achieve these goals.

In this paper, I focus on the government's political goal of maintaining social stability and study whether business groups of SOEs strategically deploy resources through internal capital markets in response to regional social unrest.[41] Governments value social stability. Failing to address social unrest properly could result in a government's loss of political power (Acemoglu and Robinson (2000b)) and lower economic growth (Alesina and Perotti (1996)). As controlling shareholders, governments could impose on SOEs the goal of addressing social unrest. More specifically, business groups allocate resources across their units by conducting transactions on internal capital markets. In this way, government owners can inject funds into SOEs located in regions affected by incidents of social unrest (e.g., terrorist attacks). When SOEs in such regions

[40] For example, Megginson, Nash, and Randenborgh (1994), La Porta and Lopez-de-Silanes (1999), Dewenter and Malatesta (2001), and Ljungqvist et al. (2017)

[41] A typical state-owned business group holds multiple SOEs.

receive additional funds, they can use these funds to distribute benefits to local residents. Government owners may accept losses in share value when the allocative strategy in state-owned business groups restores peace.

Testing whether and how SOEs internalize government's political goal of maintaining social stability poses three empirical challenges. First, such a test requires a direct measure of resource allocation within business groups to discover the channel through which government intervenes. Studies on business groups based on U.S. data rely largely on segment-level data (Berger and Ofek (1995); Shin and Stulz (1998); Hund, Monk, and Tice (2012)), which is collected annually and does not provide information on internal pricing. To document changes in resource allocation relative to the timing of incidents of social unrest, it is crucial to have both frequent measurements of intra-group resource flows and the pricing rules of each transaction. Price distortions, which are normally difficult to measure, are another important dimension for evaluating allocative distortions. Complicating matters further, the evidence shows that companies strategically disclose segment data to hide sensitive information (Berger and Hann (2007)).

Secondly, in order to infer a causal relationship between resource flows and social stability, exogenous variations in the likelihood of regional social unrest are needed, because government intervention and social unrest are jointly determined. In fact, the benefits offered by SOEs may invite a potential threat of unrest. Without a shock to the probability of social unrest, one cannot conclude that changes in the intra-group resource flows are caused by instability.

Thirdly, in order to show the social consequences of resource allocation in state-owned business groups, I need a proxy of social welfare. Typically, the literature either constructs proxies for social welfare using economic and demographic data (e.g., Jones and Klenow (2016)) or relies on surveys (e.g., Anderson and Guillory (1997)). However, proxies using economic and demographic data tend to introduce issues of endogeneity at the level of the firm, because economic performance is correlated with a firm's activities. Surveys often report data at the annual frequency, and it is difficult to obtain systematic fine-grained data over multiple years. Therefore, capturing the immediate change of welfare around shocks is a challenge.

I address these challenges in my empirical design by exploiting the unique features of the Chinese setting. First, I obtain deal-level data on related party transactions (RPTs) from the China Stock Market & Accounting Research (CSMAR) database. The data records all transactions within Chinese business groups, which allows me directly to observe resource allocation within business

groups. The China Securities Regulatory Committee (CSRC) mandates that all publicly listed firms disclose all transactions between related parties within a business group. The dataset reports pricing rules of each transaction, which enable me to differentiate transactions priced at market prices or at off-market prices. Off-market prices are mainly negotiated prices and government regulated prices that potentially involve price distortion. With all this information, I can measure resource allocation by both the amount of cash flow and the price distortion associated with each transaction. Secondly, I identify a set of arguably unanticipated adverse events that take place in different regions both within and outside of China. Examples of these events include riots over separatism, scandals involving high-ranking politicians, terrorist attacks, and escalating military tensions between China and Japan. While some of these events might be expected, their exact timing and location are difficult to anticipate, and it is this variation that I am using for identification. These events have the potential to disturb the balance of social unrest and government intervention in those cities where they occur, which allows me to identify the causal relation. Thirdly, I employ data compiled by Chen et al. (2015) to construct an index of local sentiment as an approximation for social welfare, based on a textual analysis of sampled posts from Weibo (a Chinese microblogging website, similar to Twitter).

The evidence shows that SOEs internalize the political goal of maintaining social stability. First, I find that resource allocation in state-owned business groups responds to regional shocks to social stability and follows a carrot-and-stick approach. On the one hand, a government responds to shocks by injecting more funds into SOEs located in areas affected by social unrest events than into SOEs in unaffected areas, through transactions on the internal capital market of the business groups. These transactions also display a higher level of price distortion that potentially benefits SOEs in affected areas. This is the "carrot" approach. The ratio of net cash inflow generated by intra-group transactions to firm assets increases 0.9 percentage points, or 14% of one standard deviation, more for SOEs in affected areas than for SOEs in unaffected areas around shocks. This effect corresponds to 543 million Chinese yuan (about US$84 million) in additional funds in the year following a shock, equivalent to 2% of the average annual fiscal expenditure in Chinese cities in 2011.[42] Regarding the level of price distortion, the ratio of net transactions with off-market prices that bring in funds to total number of transactions increases by 7.3 percentage points, or 13% of one standard deviation, more for SOEs in affected areas than for those in unaffected areas.

[42] The National Bureau of Statistics of China.

On the other hand, when a government is faced with a severe political conflict that threatens its ultimate legitimacy, government applies the "stick". Around the March 14, 2008 Tibet riot and the July 5, 2009 Urumqi riot, the ratio of net cash inflow to firm assets decreases by 0.5 percentage points, or 8% of one standard deviation, more for SOEs located in Tibet, Aba and Urumqi (though the magnitude of this change is smaller than that described above). This effect corresponds to a withdrawal of funds amounting to US$22 million. The ratio of net transactions with off-market prices to total number of transactions also decreases by 8 percentage points, or 16% of one standard deviation more for SOEs in affected areas. This carrot-and-stick approach matches the theory developed in the political science literature on state accommodation versus repression. The government tends to be tough and impose sanctions if its controlling position is challenged (e.g., Acemoglu and Robinson (2000a); Davenport (2007a, 2007b); Walter (2006)). The 2008 Tibet and 2009 Urumqi riots triggered punishment from government shareholders, punishment that was intended to deter future demands for greater autonomy in those regions. Resource deployment in privately owned business groups, however, is insensitive to adverse social shocks.

Secondly, regarding labor markets and firm investments, my results suggest that the injection (withdrawal) of funds is associated with increases (decreases) in labor payments, number of employees, and capital expenditure. As is shown by Besley and Persson (2011), increasing labor payments and investment levels is an important means of preventing conflicts from breaking out. The economic effect is sizable. Moving from the bottom to the top decile in the distribution of additional funds received around the time of shocks is associated with 17% higher labor payments, 3% more employees, and 18% greater capital expenditure in SOEs located in affected cities. However, no significant changes are found in non-SOEs.

Third, by addressing social unrest, SOEs' incentives deviate from shareholder value maximization, and accordingly, shareholders of SOEs experience losses in market value relative to book value. Moving from the bottom to the top decile in the distribution of additional funds received around the time of shocks is associated with 11% higher book-to-market ratio in SOEs located in affected cities, which indicates that SOEs' market values decrease relative to book values. In contrast, when it comes to value, non-SOEs are immune to these same shocks, presumably because non-SOEs lack incentives to respond with distortive resource allocations. Despite the damage to SOEs' values, additional results suggest SOEs' effort of maintaining stability may benefit local firms' performance in relatively longer term.

Finally, to infer the impact on social sentiment of SOEs' activities, I relate the city-level sentiment index to the operations of SOEs in those cities.[43] I find local sentiment deteriorates in cities that experience shocks. However, it recovers to pre-shock levels if SOEs respond to the shocks by carrying out more related party transactions aimed at such cities.

The paper contributes to four strands of literature. First, I add to the political economy literature on the interaction between governments and social unrest. To deal with social unrest, a state can either *accommodate* or *repress*, depending on which option yields higher net benefits (Acemoglu, Hassan, and Tahoun (2014); Acemoglu and Robinson (2000a, 2000b, 2001); Aidt and Franck (2015); Besley and Persson (2011); Davenport (2007a, 2007b); Walter (2006)). My paper contributes to this literature by documenting a novel channel whereby resource allocation within state-owned business groups can potentially be used by governments to neutralize social tension created by adverse shocks to stability.

Secondly, the findings in this study are closely related to a recent debate on firms' non-economic goals (Bénabou and Tirole (2010); Hart and Zingales (2017); Hsu, Liang, and Matos (2017)). I document SOEs' non-economic goal of maintaining social stability and show that the benefits offered by SOEs to the public can help restore stability.

Thirdly, this paper contributes to studies on allocative strategies in business groups. The previous literature documents the ways in which business groups can strategically deploy resources through internal capital markets to achieve expropriation (Dyck and Zingales (2004); Cheung, Rau, and Stouraitis (2006, 2010); Jiang, Lee, and Yue (2010)), alleviation of financial constraints (Buchuk et al. (2014); Almeida, Kim, and Kim (2015)), or coinsurance (Jia, Shi, and Wang (2013); Ljungqvist et al. (2017)). Using transactional-level data, I show that the political goal of maintaining social stability is a driving force of resource allocation in business groups.

Lastly, my paper speaks to political intervention in economic activities such as government procurements during times of crisis (e.g., Goldman (2017)) and regional favoritism (e.g., Hodler and Raschky (2014)). I show that governments intervene not only for economic reasons, but also for social ones. The time-varying shifts in resource allocation that I document are probably unlike the even spread of resources over time and across geographies implied by studies on regional favoritism. Overall, my paper provides new evidence on distortion in resource allocation in

[43] The sentiment index starts in August 2009. Only incidents of social unrest that occurred after 2009 can be analyzed. The events that occurred after 2009 include all events that SOEs addressed by offering benefits to local residents.

politically connected firms, and documents its social consequences (e.g., Bertrand et al. (2006); Faccio (2006); Fisman (2001); Khwaja and Mian (2005); Shleifer (1998); Shleifer and Vishny (1994)).

Section II of this paper develops testable hypotheses. Section III describes the institutional settings and introduces the shocks examined herein. Section IV provides the data, and Section V presents the empirical analysis and results. Section VI addresses robustness tests. My conclusions are presented in Section VII.

## 2.2. Theoretical framework

I develop a framework that predicts a government's response to episodes of social unrest. Government shareholders could, then, bring the strategy to SOEs and implement it as a guideline for resource management in restless times. When a government perceives a threat to social stability, it can either accommodate the threat by offering benefits or repress it by imposing sanctions.[44] Before making the decision, the government weighs the costs and benefits of the two options. While it is difficult to discuss exhaustively all possible factors in the government's assessment, I try to focus on the nature of unrest events. This focus is supported by the literature described below.

In his review paper, Davenport (2007a) points out that the most vital aspect of social unrest that a government cares about is whether the unrest challenges its political power. When social unrest challenges the status quo, an authority generally employs some form of repressive action to counter or eliminate the threat. The net benefit of repression in this case is high because failing to resolve challenges to the authority's position results in its losing control.

When the threat to social cohesion is triggered by isolated incidents that do not directly challenge its political power, governments are reluctant to engage in repression, because the benefit of such an approach is low. Davenport (2007b) looks into different types of autocracies and finds that democratic and single-party regimes are generally less repressive than other autocracies because the former two have more flexibility in promoting alternative mechanisms of socio-political control. Unlike democratic governments, which can allow individuals and groups

[44] I do not separately discuss the two extreme options where (1) the government launches a civil war or (2) full democratization is adopted. In my framework, I merge the option of civil war into state repression because sanctions and civil war share common roots (Besley and Persson (2011)), and they both mean that the government has chosen to be tough. Focusing on regional unrest, I rule out the possibility of full democratization. Democratization is ultimately an extreme form of accommodation.

to express their needs, single-party regimes can offer direct benefits to disaffected groups in order to offset social tensions. When the government imposes the above strategies on SOEs, I obtain the first set of hypotheses:

Hypothesis 1a: *If a social unrest event does not challenge an authority's regional political power, government shareholders will transfer resources to business group units located in areas that experience adverse shocks to social stability (the "carrot").*

Hypothesis 1b: *If a social unrest event challenges an authority's regional political power, government shareholders will withdraw resources from business group units located in affected areas (the "stick").*

Given the resource reallocation strategies above, one might wonder through which channels the additional resources can effectively offset social panic. Besley and Persson (2011) provide an answer: they find that higher labor payments and levels of investment reduce the likelihood of conflicts. Following this finding, it is expected that SOEs use additional resources to improve labor conditions and boost capital expenditure in order to offset social panic.

Hypothesis 2: *Employment, labor payment and levels of firm investment in SOEs are positively correlated with the net cash inflow received by SOEs in affected cities around shocks.*

Financial economists care about the efficiency of resource allocation within firms because it ultimately affects firms' value. The political objective of maintaining social stability deviates from the neoclassical efficiency of maximizing shareholders' value because it requires resources to flow into areas that are experiencing unrest and generate benefits to the local residents rather than going to profitable projects. In this end, inefficient resource deployment rises within firms, which results in the destruction of shareholder value.

Hypothesis 3: *The political objective of maintaining social stability in SOEs results in the destruction of shareholder value.*

Adverse shocks to social stability are capable of generating tension in society. When local people experience such shocks (e.g., terrorist attacks), they can become worried. The spillover effects of the shocks can damage local sentiment. However, the benefits offered by SOEs to the local residents might offset the tension and restore peace. Therefore, I hypothesize:

Hypothesis 4: *Local sentiment deteriorates in cities that experience adverse changes to stability. The presence or the response of SOEs to the events helps restore the level of sentiment.*

## 2.3. Institutional background

*A. Chinese business groups and related-party transactions*

Business groups play a major role in the Chinese economy. A typical business group in China comprises the holding company, listed firm(s) held by the controller, and many other (predominantly non-listed) subsidiaries, controlled either by the controller or by the listed firm(s). Based on the nature of ultimate controllers, Ljungqvist et al. (2017) identify in total 211 government-owned business groups and 76 privately owned groups over the period 2004–2013. While it is difficult to estimate the total size of state-owned and non-state-owned business groups, among all A share listed firms, SOEs and non-SOEs account for 42% and 58%, respectively, of the total market value (2014) and both of them are spread across mainland China.

The Chinese stock markets mandate the disclosure of related party transactions (RPTs), which are deals conducted within the same business groups. Figure 1 depicts a typical business group structure and RPTs. RPTs, as shown in the figure, enable resource allocation in the internal capital market of business groups. The details of the RPTs' data are introduced in part IV.

*B. Social stability and regional shocks*

The Chinese government emphasizes the importance of social stability in the pursuit of economic development. The concept of a "harmonious society" was brought up in 2004, in the 16th Central Committee of the Communist Party of China (CPC), and the office of the central leading group for maintaining stability (中央维护稳定工作领导小组办公室) was set up one year later. Since then, maintaining stability has become a central goal of the Chinese government (Geis, John, and Holt (2009)). The emergence of the "harmonious society" concept lays out an accommodating attitude in handling social unrest. According to the guidelines, building a "harmonious society" is a continuing process of resolving social conflicts and nipping them in the bud. Vulnerable groups should be supported and guaranteed that they will be able to benefit from social and economic

development.[45,46] SOEs are active agents in this process. The Chinese SOEs are believed to be shock absorbers in the economy. For example, SOEs could be required by the government to reserve vacancies to absorb unemployed individuals.[47] However, despite the policies, the number of episodes of social unrest has increased substantially in the last two decades in China. According to Tanner (2014), various forms of protests grow from 8,700 in 1993 to 60,000 in 2003 and more than 120,000 by 2008 as reported. The total spending on domestic security reached 514 billion Chinese yuan (roughly US$76.7 billion) in 2009, which is comparable to the military budget of 530 billion Chinese yuan.[48] The spending outgrew the military budget in subsequent years.[49] With the exception of subsidies of vulnerable groups, this spending reflects the build-up of domestic security forces such as police and surveillance facilities.

Even though almost no incident has affected stability on the national level, China does not lack for incidents that disturb social stability regionally. Regional incidents, by generating pressure on local stability, contribute to identifying the causal inference from social instability to resource deployment in SOEs. I collect salient events for the tests from the survey "Conflict Barometer," compiled by the Heidelberg Institute for International Conflict Research, an annual survey that reports conflicts globally. For each country, the report creates a set of topics describing all the relevant conflicts in the country, and then tracks all the specific events that happen each year by category. Categories are rated each year according to severity, from 1 (lowest) to 5 (highest). I focus on the highest-rated categories, which include territorial disputes, social-economy conflicts, and ethnic conflict in China.[50]

In the end, I include eight events between 2008 and 2014. The events are: (1) Japanese and Chinese boats collided near the Senkaku/Diaoyu Islands in September 2010; (2) Tibetan students protested education reform in October 2010, Qinghai province; (3) The Japanese government's

[45] J. Wang, "Build a harmonious society: Keep resolving social conflicts" (构建和谐社会：不断化解社会矛盾的过程), *People*, October 12, 2006.
[46] S. Feng, "Building a harmonious society requires solving problems among vulnerable groups" (构建和谐社会必须解决弱势群体问题), *People*, July 21, 2005.
[47] Y. Zheng, "Shocks and absorbers," *The Economist*, January 16, 2016.
[48] C. Buckley, "Analysis: China price for stability raises alarm," *Reuters*, October 14, 2010.
[49] B. Blanchard and J. Ruwitch, "China hikes defense budget, to spend more on internal security," *Reuters*, March 5, 2013.
[50] Regarding conflicts involving China since 2008, the highest-rated conflicts are, for ethnic conflicts, Tibet-Han and Uighur-Han (rated between 3 and 4); for territorial conflicts, the Senkaku/Diaoyu Island disputes (rated 2); and socioeconomic conflicts (rated between 3 and 4).

nationalization of the Senkaku/Diaoyu Islands in September 2012; (4) The Bo Xilai incident in March 2012, Chongqing; (5) A car bomb in Tian'anmen Square in October 2013, Beijing; (6) Muslim extremist groups randomly attacked civilians in Kunming, Guangzhou, Changsha, and Huaihua in March 2014; (7) The Tibet riot in March 2008; and (8) The Urumqi riot in July 2009. Table 1 gives an overview of dates, affected areas, a brief description, and media coverage of all these incidents. By selection, the eight events represent most severe social unrest episodes during the sample years, which might not be representative for unrest episodes with lower severity in China. In robustness test, I relax the selection criteria by including less severe events, and discuss the results accordingly.

These eight events can be *ex ante* classified as either "carrot" events, which the government tries to resolve by offering benefits to groups at risk; or "stick" events, which the government represses. The 2008 Tibet and 2009 Urumqi riots are potential "stick" events. Riots due to a desire for ethnic separatism represent an extreme category of political conflicts and are the type of social unrest most likely to trigger state sanctions. By demanding high-level autonomy or total independence, such riots put the local rule of the CPC at risk. According to hypothesis 1b, government shareholders are more likely to apply the "stick" in cases such as the Tibetan and the Urumqi riots. In addition, several characteristics further prevent government accommodation in the two riot cases. First, Tibetans and Uighurs are very different from the majority Han Chinese in terms of culture, such as language, religion, and lifestyle, which undermines mutual trust (Avruch (1998)). Secondly, government tends to create a reputation of toughness for itself when faced with more than one separatist group (Walter (2006)). The purpose is to set a harsh example for other groups and deter future incidents. Thirdly, the two autonomous regions receive constant subsidies from the government. For example, Wei (2015) calculates the accumulated subsidies to Tibet from 1952 to 2013 as 542 billion Chinese yuan, accounting for over 90% of Tibet's financial revenues. For Xinjiang, it is reported that the central government planned to provide support I the amount of more than 25 billion Chinese yuan after 2010.[51] Moore (2000) shows that government adopts tactics of repression when earlier accommodations are met with dissent, and of accommodation when they are accepted. In other words, government will withdraw regular subsidies to a target group when subsidies fail to prevent unrest. The remaining six events are more likely to be "carrot"

[51] Y. Wang and J. Yang, "An update on the support to Xinjiang economy" (支援新疆经济发展政策全面维新), *Caijing*, May 14, 2010.

events, either because they disturb local stability but do not challenge the rule of the authority, or because local stability is affected by either external forces (e.g., foreign countries and terrorists) or government failures (scandals involving politicians and the failure of local policies). In Session V, I conduct separated tests for the six "carrot" events jointly and the two "stick" events jointly.

Despite the *ex ante* classification according to the prediction of political science literature, I also take an agnostic approach to let data reveal how the government addresses each of the included unrest events by testing them individually in Session VI.

## 2.4. Data

I build my analysis on four main sources: (1) data on related related-party transactions; (2) firm ultimate controller information; (3) labor and financial data; and (4) local sentiment index.

*A. Related party transactions*

Data on related party transactions of Chinese listed firms is available in the CSMAR Related Party Transactions research database. CSMAR includes data on approximately 500,000 deals from 1997 to the second quarter of 2015. A related party is defined as related according to one of twelve categories: between a firm and its (1) holding company, (2) subsidiaries, (3) commonly held firms, (4) major investors, (5) joint venture/associated firms, and (6) key managers, as well as six others.[52] I only include deals conducted between entities that have common shareholding. The data cover various aspects of business between firms. I focus on six types of transactions: (1) commodities, (2) assets, (3) intra-debt, (4) guarantees, (5) equity transactions, and (6) direct transfer free of charge.[53] The data allows me to extract information about the transaction date, products, and/or services exchanged; the amount of the transaction; and pricing rules. Owing to a lack of financial and investment data regarding non-listed firms, I only include deals involving listed firms.[54] I start with the year 2007 for the data quality of related party transactions. This screening strategy leaves me with 244,723 deals from 2,551 firms. The average size of transaction in terms of Chinese yuan

[52] The CSMAR database defined 12 different categories for related parties. For conciseness, I merged them into 7 categories.

[53] The CSMAR database offers a total of 21 different categories of transactions. The six categories in my sample account for 80% of all the deals.

[54] While I am collecting non-listed firms information, listed SOEs account for significant share of overall stated owned economy. At province level, listed SOEs account for on average 60% of the total assets during the sample year. In addition, listed firms receive substantially more attention from the investors. If the goal were to maintain stability through SOEs, government would make its effort as visible as possible by working through listed SOEs.

is 123 million—roughly US$18 million. Table 2, panel A, reports the summary statistics of related party transactions.

*B. Controlling shareholders*

Data on the controlling shareholders of listed firms is obtained from the RESSET database. The dataset classifies eight different controlling shareholder types, which enables me to classify firms into central-government-owned enterprises, local-government-owned enterprises, and all the other non-state-owned enterprises.[55] I have 762 SOEs accounting for 30% of firms in my sample.

*C. Labor, investment, and financials*

From CSMAR, I collect firms' quarterly total labor payment, capital expenditure, and number of employees. All the quarterly firm financial data is from CSMAR too, such as firm equity, leverage ratio, cash-to-assets ratio, and return on assets. Summary statistics for both labor and financial data are reported in Table 2, panel B.

*D. Local sentiment*

To construct the local sentiment index, I use the Weibo Social Moods data compiled by Chen et al. (2015). Weibo is a Chinese social-media platform similar to Twitter. Because Weibo is the major social media platform in China, the content of its posts could be a reflection of local sentiment.[56] To construct the dataset, a random number of posts are scraped daily. In total, the dataset contains *1 billion* posts from August 2009 to November 2014. Each post can be traced to the city from which it originates by tracking users' IP addresses. The content of the posts is then compared to keywords indicating six emotions: Happiness, Anger, Sadness, Disgust, Fear, and Surprise. Each emotion has a list of keywords identified by the authors. There are in total 306 keywords for Happiness, 93 keywords for Anger, 205 keywords for Sadness, 142 keywords for Disgust, 72 keywords for Fear, and 27 keywords for Surprise. When a post has a match with one of the above keywords, it is scored 1 point for the corresponding emotion. The data thus provides me with daily emotion scores calculated using a given day's sampled posts across 500 cities and/or regions around the world. In my test, I focus only on 107 Chinese cities that can be matched to firms' data. The data allows me to construct the sentiment index at a city level and at a high

[55] The eight categories are (1) central-government-owned firms, (2) local-government-owned firms, (3) private firms, (4) collective firms, (5) university-affiliated firms, (6) foreign-owned firms, (7) trade-union-owned firms, and (8) other.

[56] Bollen, Mao, and Zeng (2011) argue that the tweets submitted to Twitter at any given time may provide an accurate representation of public sentiment, and they find the predictions of the Dow Jones industrial average can be significantly improved by including Twitter-based sentiment.

(quarterly) frequency. It is important to obtain a timely measure of local sentiment in my study, because low-frequency (e.g., annual) measures cannot capture the immediate change of sentiment around social unrest events.

## 2.5. Empirical analysis and results

In this part, I test the carrot-and-stick hypothesis of resource allocation in state-owned business groups. First, I examine the direction and magnitude of resource allocation within business groups. I always separate the firms sampled into SOEs and non-SOEs. Secondly, I look into the implications of the allocation strategy for the labor market, the firm's investment level, and its valuation. Finally, I associate local sentiment with the presence and activities of SOEs.

*A. Resource allocation upon adverse shocks*

Resource assignment within business groups is revealed by the related party transactions. I define two measures to capture (1) the volume of transactions in monetary terms and (2) the intensity of price distortion. To measure the volume of transactions, I construct *net inflow*, which is the net cash inflow to a firm generated by related party transactions. In each deal, a firm is either a seller or a buyer and is expected to receive or pay out a certain amount of money. I aggregate the amount of net inflow at the quarterly level for each firm. A positive net inflow, then, means that a firm receives funds on average during the quarter, and vice versa. Net inflow is scaled by the firm's total assets in the same quarter. To measure the price distortion in transactions, I define the *ratio of political deals*, which is the ratio of the net number of deals conducted following off-market prices that bring in cash to the total number of deals that occurred in a given quarter. The data provides an indicator variable and classifies pricing rules into thirteen categories, which enables me to differentiate whether deals follow market prices or not.[57] I define deals that follow market prices as "market deals" (category 1) and all the remaining deals as "political deals". Deals that do not follow market prices potentially benefit the recipients or expropriate them. The percentage of net off-market-priced deals in a given quarter, therefore, measures the intensity of price distortion in resource allocation. With this measure, I can show that even if the total amount of funds

[57] The 13 categories are (1) market price, (2) conditional market price, (3) agreed-upon price, (4) nationally regulated price, (5) conditional nationally regulated price, (6) local-government-regulated price, (7) bank interest-rate-based price, (8) ex-factory price, (9) cost-based price, (10) tender price, (11) evaluated price, (12) face-value-based price, and (13) other.

transferred does not change around a shock, the level of price distortion may increase. The summary statistics of both scaled *net inflow* and *ratio of political deals* are presented in Table 2, panel B.

With respect to shocks to regional social stability, I try to identify that resource allocation within state-owned business groups is driven by the political goal of maintaining stability. The regression follows a difference-in-differences approach. The specification is:

$$\frac{Net\ inflow_{fq}}{Total\ assets_{fq}} = \alpha_f + \alpha_q + \alpha_{area,qoy} + \beta Post_q \times City_f + \gamma' x_{fq} + \varepsilon_{fq} \qquad (1)$$

The dependent variable is the ratio of *Net inflow* to *Total assets* of firm $f$ in quarter $q$. I regress the variable on an indicator, $Post$, equal to 1 in four quarters, subsequent to each shock listed in Table 1; another indicator, $City_f$, equal to 1 if a firm is located in a city that experiences social unrest events; and the interaction term $Post_q \times City_f$. The term $x$ is a vector of firm and macroeconomic control variables, including the natural logarithm of firm equity, return on assets, leverage ratio, cash-to-asset ratio, the natural logarithms of local GDP, population, and fiscal expenses. The cities treated in each event are listed in Table 1, in the "affected area" column. Firm fixed effects, quarter fixed effects, and economic zone–quarter of the year fixed effects are included, and therefore only the coefficient of the interaction term is identified. Government policy makers designed the concept of economic zones with the aim of smoothing the promotion of the collective economy and cooperation between provinces. Controlling economic zone–quarter of the year fixed effects, therefore, helps to absorb confounding effects from the business cycle. Using the broadest definition of economic zone, China is divided into four zones. A positive $\beta$ coefficient in equation (1) indicates the "carrot", approach where firms located in affected cities receive additional funds. Otherwise, a negative $\beta$ coefficient indicates the "stick" approach. Standard errors are double-clustered at the city and quarter level.

In practice, I conduct tests by merging multiple events together and generate a continuous treated variable $Treated_{cq}$ that equals 1 in the following 4 quarters in cities after unrest breaks out, otherwise 0. Equation (1), therefore, is transformed into (1′) below:

$$\frac{Net\ inflow_{fq}}{Total\ assets_{fq}} = \alpha_f + \alpha_q + \alpha_{area,qoy} + \beta Treated_{fq} + \gamma' x_{fq} + \varepsilon_{fq} \qquad (1')$$

I use equation (1′) to test the groups of "carrot" and "stick" events separately.

In order to examine the change in the level of price distortion around each, I alternatively replace the dependent variable in (1′) with the *Ratio of political deals*:

$$Ratio\ of\ political\ deals_{fq} = \alpha_f + \alpha_q + \alpha_{area,qoy} + \beta Treated_{fq} + \gamma' x_{fq} + \varepsilon_{fq} \quad (2)$$

A positive $\beta$ coefficient in equation (2) implies that firms located in affected cities engage in more distorted transactions, while a negative $\beta$ suggests a lower price distortion in transactions.

Estimates of (1′) and (2) are reported in panels A and B of Table 3, respectively. Columns (1)–(2) present results for "carrot" events; columns (3)–(4) present results for "stick" events. According to hypothesis 1a, for "carrot" events I expect to observe that government shareholders inject funds into SOEs located in affected areas, and that the level of distortion in deals increases after shocks. Indeed, the results in column (1) in both panel A and panel B confirm my hypothesis. SOEs located in areas of unrest receive on average 0.9 percentage point more net cash inflow relative to firm assets, which is 14% of one standard deviation (6.6) more than SOEs located in areas not affected by unrest. This amount corresponds to 543 million Chinese yuan (0.9% × 15.1 billion × 4), roughly US$84 million, in the year following the shocks. The total dollar value transferred through related party transactions accounts for 2% of the total average fiscal expenditure of Chinese cities in 2011. Regarding price distortion, the ratio of net political deals to total number of deals increases by 7.3 percentage points, or 13% of one standard deviation more for SOEs in affected areas. Results for "stick" events focus on the March 14, 2008 Tibet riot and the July 5, 2009 Urumqi riot. The hypothesis predicts that government shareholders punish defiance and deter future rioting by withdrawing funds after a riot. The results in column (3) confirm hypothesis 1b. I find a significant drop in the net cash inflow and in the level of price distortion among transactions involving SOEs located in Tibet, Aba, and Urumqi. However, at a smaller magnitude, the coefficient suggests that the riots led to a decrease of 0.5 percentage point (8% of one standard deviation) in the ratio of net cash inflow to firm assets, and a decrease of 8 percentage points (16% of one standard deviation) in the ratio of political deals. The outflow of funds was on average US$22 million per SOE in the year after each riot. This suggests that the government reduces the level of subsidies to Tibet and Xinjiang economies after riots.

Regarding non-SOEs, I conduct the same analysis; the results are reported in columns (2) and (4). I expect resource allocation in non-SOEs to be insensitive to social unrest. The primary goal of non-SOEs is to generate value for their owners. On the one hand, without government shareholders, non-SOEs lack any incentive to comply with a given political objective. On the other

hand, even if the business environment becomes unstable, non-SOEs would not necessarily take away from the affected areas because they anticipate that the government (shareholders) would respond to incidents of social unrest. The cost of relocation could be too high to be a viable option. Throughout, the estimated coefficients are statistically insignificant. To the extent that I do not find resource allocation in non-SOEs to change significantly with respect to shocks, the results are in line with my expectations.

In addition, I explore two potential heterogeneous effects between different types of SOEs. Ljungqvist et al. (2017) show that CEOs in SOEs tend to comply with government's political goals because of promotion incentives. The finding implies that local-government-owned SOEs might react to local social unrest more strongly than central-government-owned SOEs. To test this cross-sectional variation within SOEs, I interact the variable $Treated_{fq}$ with a dummy variable, $Central_f$, equal to 1 if a firm is owned by the central government, and 0 otherwise. The estimates are reported in Table B.1 columns (1) and (3). Throughout, the coefficients on the interaction term are statistically insignificant. The result implies that central-government-owned SOEs respond to adverse events as strongly as do local-government-owned SOEs. Another expectation is that firms in sectors owned by the government with less external competition could display stronger effects in responding to social unrest events. Due to the lack of competition, firms in these sectors could cater more to the political pursuit of the government. To test it empirically, I classify SOEs into so-called state monopoly industries and non-state monopoly industries according to Du (2010) and Chen (2008), which identify 13 sectors as Chinese state monopoly sectors.[58] The results are reported in columns (2) and (4) in Table B.1, I find no statistically significant coefficients on the interaction term, suggesting the difference between state monopoly sectors and non-monopoly sectors in terms of address social unrest events is insignificant.

In shock-based studies, it is crucial to show that a parallel trend assumption holds between treated and controlled firms (Atanasov and Black (2016)). A parallel trend indicates that (1) results are not contaminated by any pre-treatment shocks to either firms that are treated or control firms; and (2) the treatment is not anticipated before it happens. I provide empirical support by showing

[58] The state monopoly sectors include 1) oil and gas exploration, 2) tobacco, 3) petroleum processing, 4) coking and nuclear fuel processing, 5) electricity, heat production and supply, 6) gas production and supply, 7) water production and supply, 8) railway transport, 9) postal service, 10) telecommunications and other information transmission services, 11) financial services, 12) non-ferrous metals exploration and processing, and 13) ferrous metal exploration and processing.

that resource allocation in SOEs located in affected areas only starts to change after a shock. I plot the point estimates from a modified version of equations (1′) and (2), where I introduce the cohort dimension to create control groups for each individual event and allow the effect of $Treated_{fq}$ to vary by quarter. Figure 2 suggests that, around "carrot" events, the increase in net cash inflow (graph on left) and the level of price distortion (graph on right) are significant only after the normalized event dates.

In sum, this section presents the carrot-and-stick strategy in SOEs' decisions to allocate resources with the aim of maintaining social stability. Depending on the nature of the shock, funds are injected into or taken away from SOEs in affected areas. The level of price distortion also changes around the shocks. A natural question would be: does the allocation plan have real effects on the labor market and on the firm's investment decisions?

*B. Labor market and firm investment decisions associated with resource allocation*

Hypothesis 2 states that SOEs, upon receiving (the withdrawal of) resources, increase (decrease) various benefits to employees or local residents with the purpose of neutralizing the negative effect of the shock. In practice, firms can generate or reduce benefits to the public by altering their policies in the labor market and in investment decisions. I therefore relate (1) labor payments, (2) hiring decisions (the number of employees), and (3) investment levels (capital expenditure) to the changes in funds received by SOEs around shocks. In this test, I merge both "carrot" and "stick" events together. As SOEs' policies hinge only on whether they obtain additional resources around shocks, there is no need to differentiate between "carrot" and "stick" cases. The equation is specified as:

$$Firm\ policy_{fq} =$$

$$\alpha_f + \alpha_q + \alpha_{area,qoy} + \beta_0 Treated_{fq} \times \Delta Cash_f + \beta_1 Treated_{fq} + \gamma' x_{fq} + \varepsilon_{fq} \qquad (3)$$

On the right-hand side, the variable $Treated_{fq}$ interacts with $\Delta Cash_f$, which is the change in funds received by firm $f$ around a shock. As shown in equation (4), I compute it by taking the first difference of the medians of the net cash inflow to total assets ratio in firm $f$ in shock- and non-shock periods.

$$\Delta Cash_f = Median\left(\frac{Net\ inflow_{fq}}{Total\ assets_{fq}}, Post_q = 1\right) - Median\left(\frac{Net\ inflow_{fq}}{Total\ assets_{fq}}, Post_q = 0\right) \quad (4)$$

A positive $\Delta Cash_f$, therefore, means firm $f$ receives more funds after a shock. In the regression, firm fixed effects, quarter fixed effects, and economic zone–quarter of the year fixed effects are included. A positive and statistically significant $\beta_0$ is expected for SOEs, while firm policies in non-SOEs should be insensitive to events that threaten social cohesion.

Estimates of (3) are reported in Table 4 for SOEs (odd-numbered columns) and non-SOEs (even-numbered columns). The coefficients on the interaction term in columns (1), (3), and (5) are positive and statistically significant. The results indicate that moving from the bottom to the top decile in the distribution of additional funds (1.06) received around the time of a shock is associated with 17% ($1.06 \times 0.04 \times 4$) higher labor payments, 3% ($1.06 \times 0.007 \times 4$) more employees, and 18% ($1.06 \times 0.045 \times 4$) higher capital expenditure in an SOE located in an affected city in the year following a shock.

Results of the interaction term for non-SOEs are presented in columns (2), (4), and (6), and throughout, coefficients are not statistically different from 0. It confirms that resource allocation and various firm policies in non-SOEs do not aim to address social issues.

To sum up, my finding consistently supports the prediction that SOEs use the funds received around a shock to generate benefits to employees or local residents. Specifically, SOEs increase payments for labor, capital expenditure, and the number of employees.

*C. Valuation implication for firms*

In this section, I study the valuation implication when SOEs meet the political goal of maintaining social stability. It is costly to shareholders if a firm's incentive deviates from value maximization toward the political goal. Inefficiency arises because scarce resources, rather than being deployed at the best investment opportunities, flow now to regions of unrest. To show the value implication of the carrot-and-stick strategy around a shock, I follow equation (3) and replace the dependent variable with a firm's book-to-market ratio.

I estimate the equation for SOEs and non-SOEs, respectively, and the results are presented in Table 5. In column (1), the coefficient on the interaction term $Treated_{fq} \times \Delta cash_f$ is positive and statistically significant for SOEs. Moving from the bottom to the top decile in the distribution of additional funds received around the time of a shock is associated with 11% ($1.06 \times 0.03 \times 4$ / 1.1) higher book-to-market ratio in SOEs located in affected cities, indicating lower market-to-book values. Investors indeed perceive the injection of funds into affected areas as a source of inefficiency. They are able to form this belief and trade on it because listed firms are required by

regulators to disclose related party transactions. In contrast, the coefficient on the interaction term in column (2) for non-SOEs is negative and statistically insignificant. Shareholder value for non-SOEs seems to be insulated from shocks to regional social stability and any resource allocation around them.

Overall, the results confirm hypothesis 3, along with the finding in the political connection literature that political incentives can distort resource allocation within firms, resulting in inefficiency. Given the value destruction of the carrot-and-stick strategy in SOEs, the more important question is: does a strategy that aims to maintain social stability generate positive externalities?

*D. Local sentiment and firm action*

In my study, it is important to document empirically that the benefits offered by SOEs indeed help to offset social tension caused by various kinds of shocks. To capture local sentiment across Chinese cities and measure it at the quarterly frequency, I use the "Weibo Social Moods" dataset (Chen et al. (2015)). Since the dataset ranges from August 2009 to November 2014, it allows me to conduct analysis only on "carrot" events for the coverage of both pre- and post-shock periods. I take the keyword score of "happiness" as a measure of positive sentiment. I normalize keyword scores of positive sentiments by the number of corresponding keywords used to generate the scores and the number of total posts sampled in a given quarter. The index can be expressed by the following formula:

$$Sentiment_{cq} = \frac{Happy\ keyword\ score_{cq}}{no.of\ Happy\ keywords_{cq} \times total\ no.of\ posts_{cq}} \quad (5)$$

As the test can only be conducted at the city level, I need an aggregated measure of SOE and non-SOE activities in each city. I first consider whether a city has listed SOEs or not ($SOE\ (Y/N)$). I expect sentiment to improve more in cities where listed SOEs operate, compare to other treated cities that have no listed SOEs at all. The implicit assumption is that SOEs would always implement the carrot-and-stick strategy in resource allocation. Second, more directly, I measure the relative size of related party transactions conducted by SOEs and non-SOEs in a city. To do so, I take the median of net cash inflow to total assets ratio for SOEs and non-SOEs, respectively, at the quarter and city levels. I then compute the ratio ($SOE\ RPTs$) between the two medians. A higher $SOE\ RPTs$ means SOEs conduct more RPTs in a given city-quarter. I again use the first difference between the median of $SOE\ RPTs$ at the city level to gauge the change in SOEs'

activities around a shock. I expect greater improvement of local sentiment in cities where SOEs conduct more RPTs around a shock.

The regression equation is specified as:

$$Sentiment_{cq} = \alpha_c + \alpha_q + \alpha_{area,qoy} + \beta_0 Treated_{cq} \times SOE_{cq} + \beta_1 Treated_{cq} + \beta_2 SOE_{cq} + \gamma' x_{cq} + \varepsilon_{cq} \quad (6)$$

$SOE_{cq}$ is either of the above two measures, $SOE\ (Y/N)$ or $\Delta SOE\ RPTs$. I control for the same set of macroeconomic variables, namely, local population, fiscal expenditures, and GDP. Instead of including firm fixed effects, I control city fixed effects in this city-level analysis. Table 6 presents the results. Focusing first on columns (1) and (2), the coefficients on the interaction term $Treated_{cq} \times SOE\ (Y/N)_{cq}$ are positive and statistically significant, while coefficients on the variable $Treated_{cq}$ are negative and significant. The combination indicates that social unrest events damage sentiment in the treated cities. However, the presence of listed SOEs in such cities alleviates social tension. In terms of economic magnitude, the level of happiness decreases by 17% in cities where social unrest events occur, but conditional on listed SOEs exist in such cities, the level of happiness bounces back by 19%. The results shown in columns (3) and (4) lend further support to the finding: cities where SOEs conduct more RPTs around a shock experience an improvement in sentiment. The results confirm hypothesis 4. The benefits generated by SOEs are positively associated with local sentiment. The more benefits SOEs offer, the more positive sentiment is reflected on social media. It suggests that the strategy adopted by government shareholders works for relieving tension caused by adverse events.

In addition, the change in the sentiment could be revealed slowly on social media. I, therefore, replicate the test using one-quarter forward sentiment, and controlling for the contemporaneous sentiment. Results are reported in columns (5) and (6). Coefficients are slightly bigger, indicating that the variation in sentiment is captured better with forward sentiment. It does seem to take time for the change of sentiment to reveal.

Another potential concern is that Weibo users might choose to express their true emotion only when they use virtual private network (VPN) due to government online censorship. This concern can be alleviated by two reasons: 1) I use the emotion “happiness”, which is positive, as my sentiment measure. Compared to expressing negative emotion, it is less likely that Weibo users express happiness only with VPN. 2) The usage of VPN on Weibo seems to be limited. VPN

enables Weibo users to disguise themselves with non-Chinese IP addresses, and these users will be located as "overseas". The number of overseas posts only accounts for less than 5% of the total number of posts in the "Weibo Social Mood" dataset. Considering the 5% of posts also include posts from actual overseas users, the precise number of VPN users is likely to be even smaller.

## 2.6. Additional tests

*A. Reverse causality*

In this section, I address the issue of reverse causality: knowing that government shareholders would offer benefits to maintain stability, one could threaten to create trouble and ensure that such benefits would be forthcoming. This possibility undermines the exogeneity of various events. I address the issue by testing to find whether current benefits predict future conflicts. Specifically, I conduct analysis at the city level following the procedure described in section V.D (analysis of local sentiment). The goal is to find out whether conflicts happen more often in cities where listed SOEs are located, or whether listed SOEs conduct more RPTs around a shock. I combine both labor conflicts and terrorist attacks as a proxy for overall conflict incidents in China. I obtain the number of labor conflicts at the city level from the China Labor Bulletin—Strick Map, a database that has collected data on labor conflicts in Chinese cities since January 2011. I obtain data on city-level terrorist attacks from the Global Terrorism Database. The earliest terrorist attack recorded was in 2005. For the sake of consistency, I set the start of the merged dataset as January 2011. I use all the cases, including unconfirmed events (rumors), which can be mapped into a certain city. I focus solely on labor conflicts and terrorist attacks because these two types of events in China are surveyed systematically. Ideally, of course, I would like to obtain a more comprehensive measure of conflict. The regression follows equation (6), but the dependent variable becomes the number of conflicts at (1) the current time, (2) one quarter later, and (3) two quarters later. Table B.2 reports the estimates. Throughout, the coefficients on the interaction terms are statistically insignificant, indicating that neither the presence of SOEs nor the RPTs conducted by SOEs predict future conflicts. The results alleviate the concerns about revers causality in my tests.[59]

*B. Individual events*

[59] Alternatively, I regress the number of conflicts directly on the median of an SOE's net cash inflow to the total assets ratio at the city level. The ratio is not predictive with respect to future conflicts.

In the main test, I classify the eight events *ex ante* into "carrot" events and "stick" events. Even though the prediction closely follows conclusion from the political science literature, the application could be questioned in the Chinese context. To lend support to the *ex ante* clarification, I test how SOEs respond to the eight social unrest events individually. In this approach, I let data reveal government shareholders' strategies. The results are reported in Table B.3 in the appendix. All the signs of the coefficients are in line with expectation. Namely, all tests on individual "carrot" events obtain positive coefficients, and tests on individual "stick" events obtain negative ones.

*C. Alternative channels*

Conflict resolution is a topic that concerns political, economics, and legal studies. To my best knowledge, there is no literature that review all potential channels that government can adopt to address social unrest across the three disciplines. I therefore first provide my incomplete summary, and then test one major direct channel. Finally, I discuss the relevance of the SOEs channel I discover. Merging discussion in the political science and the economics studies, a government can accommodate and/or repress unrest events. Major forms of state accommodation include democratization (e.g. Acemoglu and Robinson (2000a)) which enables citizen to express their needs within the political system, and the provision of direct benefits, which could range from selective incentives, wages, bribes, fiscal redistribution to welfare systems (Davenport (2007b). In terms of state repression, mostly cited channels include both political and economic sanction, harassment, surveillance/spying, arrests, torture, and mass killing in the extreme (Davenport (2007a). The legal procedures to resolve conflicts and disputes are primarily negotiation, mediation, arbitration, and litigation. Compared to all the channels raised, the channel that government uses SOEs to address social unrest events is an indirect one. However, as Davenport (2007a) mentions, the use of alternative channels by government to address conflicts is not examined extensively, and it is important to investigate because alternative channels could help maintain political order.

Despite my results suggesting the channel of SOEs is important and effective, I test one direct channel: fiscal redistribution around social unrest events. I focus only on this direct channel due to the lack of data on state surveillance/spying, harassment, or arrests, etc. Instead of using the network of government-owned business groups, the government could resolve threats to social stability by offering subsidies to everyone in affected cities directly through fiscal budgets. I show that the fiscal redistribution channel exists along the channel of strategic resource allocation in

SOEs. I use the amount of "civil affair expenses" disclosed by the Chinese government at province and year level as a proxy for general social-welfare benefits provided by the government to the public regionally. Civil affair expenses are moneys spent by the government to guarantee civilians' basic welfare; it includes expenses for social welfare, a minimum living allowance, resettlement funds, etc.

I test the change of civil-affairs expenses relative to the GDP around shocks to social stability. The results are reported in Table B.4. The analysis is conducted at the province and year levels. I obtain positive coefficients. Based on the coefficient in column (3), the annual ratio of civil-affairs expenses to GDP increases 11.3% (0.096 / 0.85) in the provinces that experience shocks to social stability. The results indicate that the government employs multiple tools simultaneously to maintain social stability. Comparing the magnitude between the two channels, the results in Table B.4 suggest government transfer 241 million USD on average into affected provinces after shocks, while according to Table 3, one SOE transfers 84 million USD on average. The comparison shows that the SOEs channel is material.

*D. Firm performance*

Results in Session V.C show that SOEs incur immediate value destruction for addressing social unrest. However, it is not clear if maintaining social stability damages firm performance in the immediate future. On the one hand, shifting resources is costly for firms during social unrest, which could damage firm performance. However, on the other hand, stability allows firms to operate safely, and could contribute to future profits generation. Empirically, I replicate estimation in Session V.C, and replace the dependent variables as half-year or one-year forward return on assets (ROA). Results are reported in Table B.5. The positive and significant coefficient on the interaction term in columns (1) indicates that related party transactions conducted during social unrest is associated with increased profitability for the SOEs in two quarters after the shocks. However, the effect becomes insignificant one year later as results shown in column (3). For non-SOEs, results in column (2) and (4) show that non-SOEs in treated areas experience an increase in profitability two quarters and one year after the shocks, which does not depend on any related party transactions. It suggests non-SOEs may benefit from SOEs' increased investment. The results confirm, to some extent, that maintaining stability may benefit business growth.

*E. Price distortion*

In the test of price distortion in Table 3, an implicit assumption is that the prices in the political deals are distorted toward the benefits of the recipient SOEs. However, this may not be the case in practice. If prices are set in such a way that SOEs are overcharged, then the increase of political deals around shocks indicates not the injection of resources but their withdrawal. To address this concern, I focus on the percentage of guarantees deals provided to SOEs. By providing guarantees to the SOEs, the business groups offer collaterals and bear ultimate responsibility for repayment of the loans. SOEs could then potentially obtain loans that could not be obtained otherwise. The guarantees themselves are also severely mispriced and seem like free money.[60] To some extent, the guarantees represent a form of distortion that is beneficial to the receivers. Therefore, I compute the ratio of the net number of guarantees received by the SOEs to the total number of deals in each quarter and test how it changes around shocks. The results are reported in Table B.6. The test follows exactly the specifications in Table 3, and I obtain results here that are very similar to those in Table 3. In particular, with respect to the subset of guarantees deals, the results suggest that distortion increases around "carrot" events and favors SOEs. Accordingly, distortion decreases around "stick" events and disadvantages SOEs.

*F. More events*

The eight social unrest events are more severe ones during the sample period. Consequently, results based on these events might not be applicable to unrest events with lower severity. To investigate government shareholders' respond to less severe unrest episodes, I replicate the test in Table 3 by including social unrest events with lower severity according to the survey. The relaxation in selection criteria increases the number of "carrot" events from 6 to 25.[61] The number of "stick" events remains the same. The results for "carrot" events are reported in Table B.7. The coefficients have the same signs and similar level of significance as those in Table 3. However, the economic magnitude reduces. On the one hand, the carrot-and-stick strategy is robust to including less severe unrest events. On the other hand, the reduced economic significance indicates that the government shareholders tend to use SOEs to address the most disturbing unrest episodes, possibly due to the large consumption of firm resources and the damage to firm values.

[60] J. Zhang, "Credit default swaps are storing up trouble for China," *Financial Times*, August 30, 2017.

[61] The 25 social unrest events include episodes with detailed information on the events and identifiable location with severity rated at 2 and above on the survey. Nevertheless, these events are still of high significance and visibility internationally. For the newly included events, there are 4 subnational predominance conflicts, 8 socioeconomic protests, 5 peasants protests, 1 religion/ideology conflict, and 1 territorial dispute.

## 2.7. Conclusion

Relying on arguably unexpected shocks to regional stability, I find evidence that resource allocations within government-owned business groups respond to that threat of social stability at a cost of firm value. Government shareholders adopt a carrot-and-stick approach: On the one hand, the government gives a "carrot" by injecting funds into firms located in areas of unrest, funds that firms use to increase payments to employees, boost investment levels, and hire more employees. On the other hand, in cases of riots involving separatist movements, the government applies a "stick" by withdrawing funds. The switch in strategies matches theories from studies on state repression: political conflicts that challenge the rule of the incumbent government invite sanctions in various forms. It also matches the "reputation-building" hypothesis: governments tend to build a reputation of toughness when dealing with separatists, especially when there is more than one minority group involved. I also show that various benefits provided by government shareholders around shocks help to recover local sentiment. My paper provides new evidence that resource allocation in firms can deviate away from the neoclassical approach to fulfill political goals, and that SOEs can serve as a political governance tool for government to manage society.

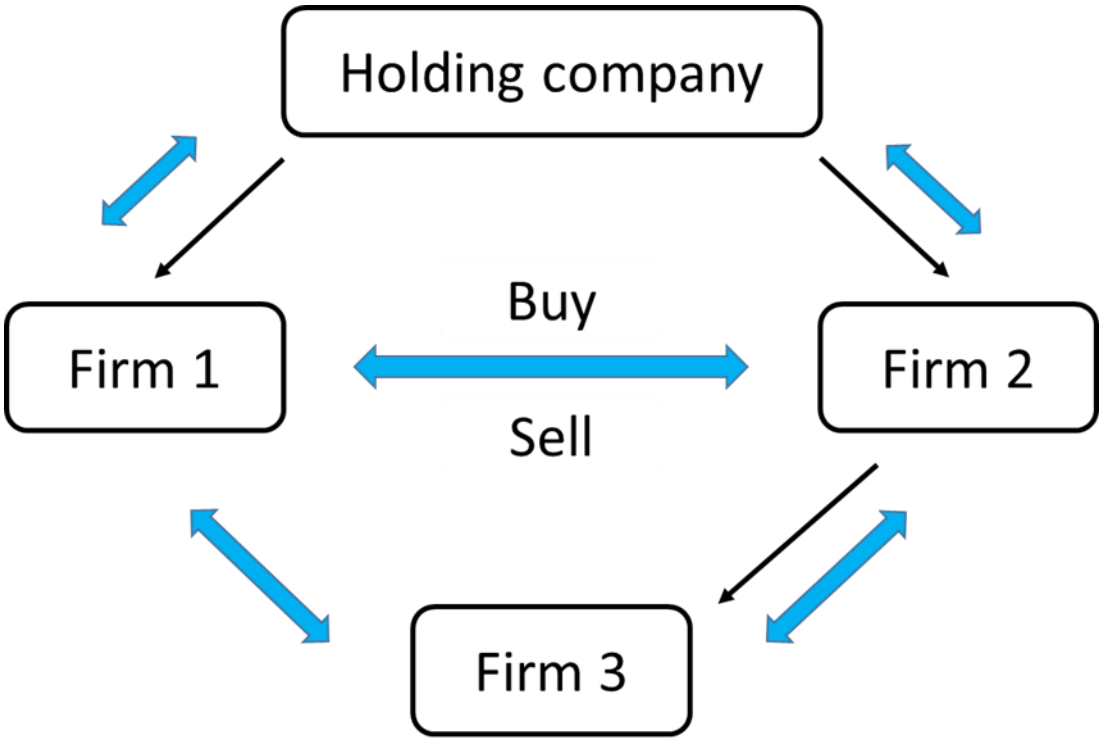


**Figure 1 Hypothetical Business Group Structure and Related Party Transactions**
The black arrows represent the direction of ownership, and blue arrows depict the direction of potential related party transactions. In my sample, I focus on transactions that involve listed firms for limited information on non-listed firms. Relevant information on related party transactions includes trade entities, cash flow, trading targets, announcement dates, trade dates, pricing rules, etc.


**Figure 2 Resource Allocation by Quarter**

This figure plots the coefficient estimates from regressing the two measures of resource allocation (net cash inflow to total asset ratio, and the ratio of political deals) on an indicator for treatment. The regression follows a modified version of equations (1′) and (2), where I introduce the cohort dimension to create control groups for each individual event and alter the fixed effects as year-by-cohort, firm-by-cohort, and area-by-quarter-by-cohort fixed effects.

**Table 1 Adverse Shocks to Regional Stability**

The table lists eight adverse shocks to regional stability. They can be classified into 3 categories: (1) territorial disputes, 2) socioeconomic conflicts, and (3) ethnic conflicts.

| Category | Time | Event | Affected Area | Description | Nature | Notes |
|---|---|---|---|---|---|---|
| **Territorial dispute: China vs. Japan** | **2010q3** | **Boats from both sides clashed near Senkaku/Diaoyu Islands** | **Fujian** | A PRC fishing trawler allegedly collided with a Japanese coast guard ship north of the disputed islands. The Chinese crew was arrested, resulting in the cancellation of bilateral dialog. | **Carrot** | "Rising tension" —BBC, Sept. 8, 2010 |
| **Territorial dispute: China vs. Japan** | **2012q3** | **Japanese government nationalized Senkaku/Diaoyu Islands** | **Fujian** | The Japanese government purchased the island on September 11, which invited strong protests from the Chinese. Six more Chinese Marine Surveillance patrols were sent to the disputed areas. The Chinese government further condoned a boycott of Japanese products and suspended the 40th anniversary of Sino-Japanese relations. | **Carrot** | "The most serious for Sino-Japanese relations in the post-war period in terms of the risk of militarized conflict." —BBC, Feb. 8, 2013 |
| **Socioeconomic conflict: Political scandal** | **2012q1** | **Bo Xilai incident** | **Chongqing** | High-ranking government politician; party chief of Chongqing; legacy of Bo Yibo, a top 25 senior party official; high flyer tipped for top office. He and his family were prosecuted on charges of bribery, abuse of power, corruption, and murder. | **Carrot** | "China's biggest political scandal" —BBC, Nov. 11, 2013 |
| **Ethnic conflict: Uighur-Han** | **2013q4** | **Car bomb in Tiananmen Square** | **Beijing** | Car bomb suicide attack initiated by the East Turkestan Islamic Movement, or Turkistan Islamic Party; 5 killed, 38 injured. | **Carrot** | "Major incident" —CN government "Unstable society" —Yomiuri Shimbun Attack "in the most politically sensitive place in China" —BBC, Oct. 30, 2013 |

| | | | | | | |
|---|---|---|---|---|---|---|
| **Ethnic conflict: Uighur-Han** | **2014q1** | **Random attack in public areas** | **Kunming, Changsha, Huaihua, and Guangzhou** | Violent attacks in public areas by the East Turkestan Islamic Movement, or Turkistan Islamic Party. | **Carrot** | |
| **Ethnic conflict: Tibet-Han** | **2010q4** | **Education reform in Tibetan schools** | **Qinghai** | Over 9,000 students protest against language-education reform promoting Mandarin over Tibetan. | **Carrot** | |
| **Ethnic conflict: Tibet-Han** | **2008q1** | **Mar. 14, Tibet riot** | **Tibet, and Aba** | Starts with street protests by monks and later descends into rioting, burning, looting, and ethnic killing of Hui and Han; 18 killed, 382 injured. | **Stick** | World attention, massive protest against Beijing Olympics |
| **Ethnic conflict: Uighur-Han** | **2009q2** | **July 5, Urumqi riot** | **Urumqi** | Protests turn violent when thousands of Uighurs attack Han and rampage through Urumqi. According to state media, the riots left 197 people dead and 1,721 injured, while the World Uighur Congress (WUC) estimates that as many as 600 died. | **Stick** | |

**Table 2 Summary Statistics**

Panel A reports a summary of statistics on related party transactions in my sample; all figures are expressed in thousands of Chinese yuan in Panel A. Panel B provides a summary of statistics for variables at the firm level; all figures are expressed in millions of Chinese yuan.

| A. Related Party Transaction (Chinese Yuan 1000) | | | | | |
|---|---|---|---|---|---|
| | **N** | **Mean** | **St. dev.** | **Min** | **Max** |
| Transaction amount | 244,723 | 123,329 | 359,258 | 3 | 2,742,000 |

| B. Firm-Level Variables (Chinese Yuan 1 million) | | | | | |
|---|---|---|---|---|---|
| | **N** | **Mean** | **St. dev.** | **Min** | **Max** |
| Net cash inflow to firm assets (%) | 63,002 | -0.03 | 7 | -28 | 30 |
| Political deal ratio (%) | 64,262 | -0.80 | 54 | -100 | 100 |
| Number of employees | 75,179 | 2,715 | 3,447 | 145 | 14,137 |
| Total labor payment | 67,860 | 119 | 841 | .33 | 40,302 |
| Capital expenditure | 65,175 | 153 | 1,880 | 0 | 132,114 |
| Book-to-market ratio | 60,506 | 0.92 | 1.32 | 0.10 | 26 |
| Firm assets | 63,008 | 8,098 | 26,687 | 150 | 265,557 |
| Firm equity | 61,719 | 2,982 | 6,577 | 0 | 58,912 |
| Leverage (%) | 62,607 | 17 | 16 | 0 | 68 |
| Cash-to-firm assets (%) | 63,000 | 20 | 16 | 0.13 | 72 |
| Return on assets (%) | 62,997 | 3 | 5 | -15 | 19 |

**Table 3 Resource Allocation at Adverse Shocks—SOEs and Non-SOEs**
The table reports the estimates of:

$$Resource\ allocation_{fq} = \alpha_f + \alpha_q + \alpha_{area,qoy} + \beta Treated_{fq} + \gamma' x_{fq} + \varepsilon_{fq}$$

In panel A, the dependent variable is the total net cash inflow scaled by the total assets of firm $f$ in quarter $q$. In panel B, the dependent variable is the percentage of net off-market-priced deals (political deals) of firm $f$ in quarter $q$. On the right hand side, $Treated$ is a continuous treated variable equal to 1 in the four quarters after an adverse shock breaks out in a city where firms are located, and $x$ is a vector of control variables, including the natural logarithm of firm equity, return on assets, leverage ratio, cash-to-assets ratio, the natural logarithm of local GDP, population, and government fiscal expenses. Columns (1)–(2) report results based on "carrot" events for SOEs and non-SOEs, respectively. Columns (3)–(4) report results based on "stick" events for SOEs and non-SOEs, respectively. All columns include firm fixed effects, quarter fixed effects, and the product of economic zones and quarter of the year fixed effects. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| Panel A | | | | |
|---|---|---|---|---|
| **Net flow / total assets** | Carrot Events | | Stick Events | |
| | (1) | (2) | (3) | (4) |
| | SOE | Non-SOE | SOE | Non-SOE |
| $Treated_{fq}$ | 0.858** | 0.167 | -0.489* | -0.271 |
| | (0.36) | (0.21) | (0.24) | (0.38) |
| Controls | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y |
| N | 15,932 | 29,693 | 8,024 | 8,501 |
| $R^2$ | 0.21 | 0.20 | 0.18 | 0.17 |

| Panel B | | | | |
|---|---|---|---|---|
| **Political deals / total deals** | Carrot Events | | Stick Events | |
| | (1) | (2) | (3) | (4) |
| | SOE | Non-SOE | SOE | Non-SOE |
| $Treated_{fq}$ | 7.329*** | 0.755 | -8.019** | -0.795 |
| | (1.93) | (1.47) | (2.99) | (5.76) |
| Controls | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y |
| N | 15,932 | 29,693 | 8,024 | 8,501 |
| $R^2$ | 0.29 | 0.25 | 0.24 | 0.26 |

**Table 4 Labor Costs, Employees, Investment Decisions, and Resource Allocation**
The table reports the estimates of:

$$Firm\ policy_{fq} = \alpha_f + \alpha_q + \alpha_{area,qoy} + \beta_0 Treated_{fq} \times \Delta Cash_f + \beta_1 Treated_{fq} + \gamma' x_{fq} + \varepsilon_{fq}$$

The sample contains both "carrot" and "stick" events. The dependent variable is the natural logarithm of labor payments (columns (1)–(2)), the log-amount of capital expenditure (columns (3)–(4)), or log-number of employees (columns (5)–(6)) in firm $f$ in quarter $q$. On the right-hand side, $Treated$ is a continuous treated variable equal to 1 in the four quarters after an adverse shock breaks out in a city where firms are located. $\Delta cash$ is the change in the median of net cash inflow to total asset ratio between event period and event-free period for firm $f$. All specifications include $x$, which is a vector of control variables used in Table 3. Columns (1), (3), and (5) report results for SOEs. Columns (2), (4), and (6) report results for non-SOEs. All columns include firm fixed effects, quarter fixed effects, and the product of economic zones and quarter of the year fixed effects. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| **Dep. variable:** | $\log(1 + Labor\ payment)$ | | $\log(1 + Capital\ expenditure)$ | | $\log(1 + No.of\ employees)$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | SOE | Non-SOE | SOE | Non-SOE | SOE | Non-SOE |
| $Treated_{fq} \times \Delta cash_f$ | 0.040** | -0.001 | 0.045** | 0.005 | 0.007** | 0.004 |
| | (0.02) | (0.01) | (0.02) | (0.02) | (0.00) | (0.00) |
| $Treated_{fq}$ | 0.013* | 0.040** | 0.111* | -0.010 | 0.054*** | 0.057*** |
| | (0.01) | (0.02) | (0.06) | (0.03) | (0.01) | (0.02) |
| Controls | Y | Y | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y | Y | Y |
| N | 21,999 | 35,401 | 21,338 | 33,881 | 22,003 | 35,418 |
| $R^2$ | 0.90 | 0.87 | 0.72 | 0.62 | 0.90 | 0.90 |

**Table 5 Firm Value and Resource Allocation**
The table reports the estimates of:

$$Book\ to\ market\ ratio_{fq} = \alpha_f + \alpha_q + \alpha_{area,qoy} + \beta_0 Treated_{fq} \times \Delta Cash_f + \beta_1 Treated_{fq} + \gamma' x_{fq} + \varepsilon_{fq}$$

The sample contains both "carrot" and "stick" events. The dependent variable is the book-to-market ratio of firm $f$ in quarter $q$. On the right-hand side, $Treated$ is a continuous treated variable equal to 1 in the four quarters after an adverse shock breaks out in a city where firms are located. $\Delta cash$ is the change in the median of net cash inflow to total asset ratio between event period and event-free period for firm $f$, and $x$ is a vector of control variables used in Table 3. Column (1) reports results for SOEs. Column (2) reports results for non-SOEs. All columns include firm fixed effects, quarter fixed effects, and the product of economic zones and quarter of the year fixed effects. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | (1) | (2) |
|---|---|---|
| | SOE | Non-SOE |
| $Treated_{fq} \times \Delta cash_f$ | 0.031** | -0.004 |
| | (0.01) | (0.01) |
| $Treated_{fq}$ | 0.002 | -0.008 |
| | (0.03) | (0.02) |
| Controls | Y | Y |
| Quarter f.e. | Y | Y |
| Firm f.e. | Y | Y |
| Area × QoY f.e. | Y | Y |
| N | 21,540 | 34,168 |
| $R^2$ | 0.81 | 0.79 |

**Table 6 Local Sentiment and SOEs Payout**
The table reports the estimates of:

$$Sentiment_{cq} = \alpha_c + \alpha_q + \alpha_{area,qoy} + \beta_0 Treated_{cq} \times SOE_{cq} + \beta_1 Treated_{cq} + \beta_2 SOE_{cq} + \gamma' x_{cq} + \varepsilon_{cq}$$

Due to data limitation, the sample only include the "carrot" events. The dependent variable is the index for local sentiment generated based on the happiness emotion score from the "Weibo Social Moods" data in columns (1)-(4), or the one quarter forward sentiment index in columns (5)-(6). On the right-hand side, $Treated$ is a continuous treated variable equal to 1 in the four quarters after an adverse shock breaks out in a city where firms are located. $SOE$ is a variable measuring the relative operation intensity between SOEs and non-SOEs in cities. It is defined either as a dummy variable that equals 1 if there is any listed SOE in a given city and quarter, or 0 otherwise (columns (1), (2) and (5)), or as a change in the relative intensity of conducting related party transactions between SOEs and non-SOEs in a given city (columns (3), (4) and (6)); $x$ is a vector of local macroeconomic control variables in columns (2), (4) and (6), including the natural logarithm of local GDP, population, and government fiscal expenses. All columns include city fixed effects, quarter fixed effects, and the product of economic zones and quarter of the year fixed effects. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| **Dep. Variable:** | ***Sentiment*** | | | | **One quarter forward *Sentiment*** | |
|---|---|---|---|---|---|---|
| | $SOE\ (Y/N)$ | | $\Delta SOE\ RPTs$ | | $SOE\ (Y/N)$ | $\Delta SOE\ RPTs$ |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Treated_{cq} \times SOE_{cq}$ | 0.024** | 0.023* | 0.001** | 0.001** | 0.028*** | 0.002*** |
| | (0.01) | (0.01) | (0.00) | (0.00) | (0.01) | (0.00) |
| $Treated_{cq}$ | -0.020* | -0.020* | -0.004 | -0.004 | -0.023** | -0.003 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.00) |
| $SOE_{cq}$ | -0.106 | -0.103 | - | - | -0.005 | - |
| | (0.08) | (0.08) | - | - | (0.01) | - |
| $Sentiment_{cq}$ | - | - | - | - | 0.160*** | 0.162*** |
| | - | - | - | - | (0.05) | (0.05) |
| Controls | N | Y | N | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y | Y | Y |
| City f.e. | Y | Y | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y | Y | Y |
| N | 4,541 | 4,541 | 2,311 | 2,311 | 4,326 | 2,203 |
| $R^2$ | 0.38 | 0.39 | 0.32 | 0.33 | 0.38 | 0.32 |

# Appendices

# Appendix 2.A.

## Variable Definitions

| Variable | Definition |
|---|---|
| *Net inflow* | The sum of net inflow of cash generated by related party transactions for firm *f* in a given quarter $q$ |
| *Ratio of political deals* | The ratio of the net number of political deals that bring in funds over total number of deals for firm *f* in a given quarter $q$ |
| *Labor payments* | The total cost of employees for firm *f* in a given quarter $q$ |
| *Capital expenditure* | The total amount of gross capital expenditure in firm *f* in a given quarter $q$ |
| *Number of employees* | The total number of employees in firm *f* in a given quarter $q$ |
| *Book-to-market ratio* | The book to market ratio of firm *f* in a given quarter $q$ |
| *Sentiment* | The keyword score of Happiness scaled by the number of Happiness keywords and the total number of posts in a given quarter $q$ $Sentiment_{cq} = \frac{Happy\ keyword\ score_{cq}}{no.of\ Happy\ keywords_{cq} \times total\ no.of\ posts_{cq}}$ |
| *City* | Dummy variable that equals 1 if firm *f* is located in shock-area(s), or 0 otherwise |
| *Post* | Time dummy variable that equals 1 in the four quarters following each shock, and 0 otherwise. |
| *Firm assets* | Total assets of firm *f* in a given quarter $q$ |
| *Firm equity* | Total equity of firm *f* in a given quarter $q$ |
| *Return on assets* | The ratio of total profits to total assets of firm *f* in a given quarter $q$ |
| *Leverage ratio* | The ratio of the sum of short-term and long-term debt to total assets of firm *f* in a given quarter $q$ |
| *Cash-to-assets ratio* | The ratio of the cash holdings to total assets of firm *f* in a given quarter $q$ |
| $SOE\ (Y/N)$ | Dummy variable that equals 1 if there is at least 1 SOE located in a city $c$ quarter $q$, or 0 otherwise |
| $SOE\ RPTs$ | The relative intensity of related party transactions conducted between SOEs and non-SOEs in a city $c$ |
| *Unrest* | The natural logarithm of the number of labor conflicts and terrorist attacks in a city $c$, quarter $q$ |
| $Civil\ affairs\ cost\ to\ GDP\ ratio$ | The ratio of civil affairs costs to local GDP at province p, year y. The civil affairs costs include government expenditures on social welfare, minimum living allowance, settlement funds, etc. |
| $Ratio\ of\ guarantees\ deals$ | The ratio of number of net guarantees deals that bring in funds to the total number of deals for firm *f* in a given quarter $q$ |

| | |
|---|---|
| *Central* | Dummy variable that equals 1 if a SOE is owned by the central government, or 0 otherwise |
| *Monopoly* | Dummy variable that equals 1 if a firm belongs to the 13 sectors that are classified as state monopoly sectors, or 0 otherwise. The 13 sectors include 1) oil and gas exploration, 2) tobacco, 3) petroleum processing, 4) coking and nuclear fuel processing, 5) electricity, heat production and supply, 6) gas production and supply, 7) water production and supply, 8) railway transport, 9) postal service, 10) telecommunications and other information transmission services, 11) financial services, 12) non-ferrous metals exploration and processing, and 13) ferrous metal exploration and processing. |
| $\Delta Cash$ | The change in funds received by firm $f$ around a shock. It is computed by taking the first difference of the medians of the net cash inflow to total assets ratio in firm $f$ in shock- and non-shock periods. $$\Delta Cash_f = Median\left(\frac{Net\ inflow_{fq}}{Total\ assets_{fq}}, Post_q = 1\right) - Median\left(\frac{Net\ inflow_{fq}}{Total\ assets_{fq}}, Post_q = 0\right)$$ |
| *Local population* | Annual population statistics at the province level |
| *Local GDP* | Annually GDP statistics at the city level |
| *Local fiscal expenditure* | Annual government fiscal-expenditure statistics at the city level |

# Appendix 2.B.

## Additional Tests

**Table B.1 Resource Allocation at Adverse Shocks— Heterogeneous Effects among SOEs**
The table reports the estimates following specifications in Table 3 with additional variables $Central$ and the interaction term $Treated \times Central$ in columns (1) and (3), and $Monopoly$ and the interaction term $Treated \times Monopoly$ in columns (2) and (4). All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| Panel A | | | | |
|---|---|---|---|---|
| **Net flow / total assets** | Carrot Events | | Stick Events | |
| | (1) | (2) | (3) | (4) |
| | SOE | SOE | SOE | SOE |
| $Treated_{fq}$ | 0.865** | 0.960* | -0.658 | -0.422 |
| | (0.42) | (0.54) | (0.57) | (0.25) |
| $Treated_{fq} \times Central_f$ | -0.054 | | 1.118 | |
| | (0.67) | | (1.15) | |
| $Treated_{fq} \times Monopoly_f$ | | -0.383 | | -0.427 |
| | | (0.74) | | (0.81) |
| Controls | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y |
| N | 15,932 | 15,932 | 8,024 | 8,024 |
| $R^2$ | 0.21 | 0.21 | 0.18 | 0.18 |

| Panel B | | | | |
|---|---|---|---|---|
| **Political deals / total deals** | Carrot Events | | Stick Events | |
| | (1) | (2) | (3) | (4) |
| | SOE | SOE | SOE | SOE |
| $Treated_{fq}$ | 7.479*** | 7.966*** | -11.067 | -6.616* |
| | (2.42) | (1.66) | (7.78) | (3.36) |
| $Treated_{fq} \times Central_f$ | -1.204 | | 20.143 | |
| | (6.31) | | (26.52) | |
| $Treated_{fq} \times Monopoly_f$ | | -2.386 | | -8.887 |
| | | (3.77) | | (12.64) |
| Controls | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y |
| N | 15,932 | 15,932 | 8,024 | 8,024 |
| $R^2$ | 0.29 | 0.29 | 0.24 | 0.24 |

**Table B.2 Resource Allocation and Future Unrest**
The table reports the estimates of:

$$Unrest_{cq} = \alpha_c + \alpha_q + \alpha_{area,qoy} + \beta_0 Treated_{cq} \times SOE_{cq} + \beta_1 Treated_{cq} + \beta_2 SOE_{cq} + \gamma' x_{cq} + \varepsilon_{cq}$$

Due to data limitation, the sample only contains "carrot" events. The dependent variable is the natural logarithm of the number of labor conflicts and terrorist attacks in a city $c$ quarter $q$. The data on labor conflicts is retrieved from the China Labor Bulletin, and data on terrorist attacks is collected from the Global Terrorism Database. On the right-hand side, $Treated$ is a continuous treated variable equal to 1 in the four quarters after an adverse shock breaks out in a city where firms are located. $SOE$ is a variable measuring the relative operation intensity between SOEs and non-SOEs in cities. It is defined either as a dummy variable that equals 1 if there is any listed SOE in a given city and quarter, or 0 otherwise (columns (1)–(3)), or as a change in the relative intensity of conducting related party transactions between SOEs and non-SOEs in a given city (columns (4)-(6)); $x$ is a vector of local macroeconomic control variables including the natural logarithm of local GDP, population, and government fiscal expenses. All columns include city fixed effects, quarter fixed effects, and the product of economic zones and quarter of the year fixed effects. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | $SOE\ (Y/N)$ | | | $\Delta SOE\ RPTs$ | | |
|---|---|---|---|---|---|---|
| | $Unrest_{cq}$ | $Unrest_{cq+1}$ | $Unrest_{cq+2}$ | $Unrest_{cq}$ | $Unrest_{cq+1}$ | $Unrest_{cq+2}$ |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Treated_{fq} \times SOE_{cq}$ | -0.272 | -0.230 | -0.123 | 3.433 | 0.971 | 0.019 |
| | (0.25) | (0.31) | (0.36) | (13.09) | (14.18) | (0.03) |
| $Treated_{fq}$ | 0.161 | 0.187 | 0.108 | 0.028 | 0.066 | 0.049 |
| | (0.21) | (0.12) | (0.14) | (0.20) | (0.25) | (0.06) |
| Controls | Y | Y | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y | Y | Y |
| N | 1,059 | 1,197 | 1,197 | 423 | 484 | 484 |
| $R^2$ | 0.65 | 0.64 | 0.64 | 0.67 | 0.65 | 0.65 |

**Table B.3 Resource Allocation at Adverse Shocks—Individual Events**

The table reports the estimates following specification in Table 3. Tests are conducted on individual events without *ex ante* merging. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| Panel A | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Net flow / total assets** | Territorial disputes: China vs. Japan 2010q3 | Territorial disputes: China vs. Japan 2012q3 | Bo Xilai incident 2012q1 | Auto bomb on Tiananmen Square 2013q4 | Random attack in public areas 2014q1 | Education reform in Tibetan schools 2010q4 | Tibet riot 2008q1 | Urumqi riot 2009q2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | SOE | SOE | SOE | SOE | SOE | SOE | SOE | SOE |
| $Treated_{fq}$ | 1.172 | 1.553** | 3.425*** | 0.604** | 0.116 | 2.345*** | -0.070 | -0.195 |
| | (0.75) | (0.75) | (0.18) | (0.24) | (0.70) | (0.25) | (0.25) | (0.21) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 5,256 | 5,613 | 5,540 | 5,725 | 5,770 | 5,314 | 4,872 | 5,013 |
| $R^2$ | 0.26 | 0.30 | 0.31 | 0.29 | 0.28 | 0.27 | 0.20 | 0.25 |

| Panel B | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Political deals / total deals** | | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | SOE | SOE | SOE | SOE | SOE | SOE | SOE | SOE |
| $Treated_{fq}$ | 7.616 | 6.018 | 7.565*** | 6.270*** | 12.349*** | 10.820*** | -18.674*** | -15.243*** |
| | (4.68) | (6.09) | (1.54) | (1.68) | (4.08) | (1.74) | (2.07) | (1.55) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 5,256 | 5,613 | 5,540 | 5,725 | 5,770 | 5,314 | 4,872 | 5,013 |
| $R^2$ | 0.38 | 0.38 | 0.40 | 0.38 | 0.38 | 0.37 | 0.29 | 0.31 |

**Table B.4 Alternative Policy Tools at Adverse Shocks**
The table reports the estimates of:

$$Alternative\ channels_{py} = \alpha_p + \alpha_y + \beta_0 Treated_{py} + \varepsilon_{py}$$

The sample contains both "carrot" and "stick" events. The dependent variable is the ratio of civil affairs cost to GDP in province $p$, year $y$ in columns (1)–(3). The civil affairs cost is the sum of government expenditures on social welfare, minimum living allowance, settlement funds, etc. On the right-hand side, the continuous treated variable $Treated$ is adapted to the province-year level. It equals 1 in the first year after an adverse shock breaks out in a province. Specification in columns (2) includes province fixed effects. Specification in columns (3) includes year and province fixed effects. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the province level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| All Events | | | |
|---|---|---|---|
| **Dep. Variable:** | $\boldsymbol{Civil\ affairs\ cost\ to\ GDP\ ratio}$ | | |
| | (1) | (2) | (3) |
| $Treated_{py}$ | 0.141* | 0.138* | 0.096 |
| | (0.07) | (0.07) | (0.07) |
| Intercept | 0.834*** | 0.359*** | 0.904*** |
| | (0.08) | (0.00) | (0.02) |
| Year f.e. | N | N | Y |
| Province f.e. | N | Y | Y |
| N | 279 | 279 | 279 |
| $R^2$ | 0.02 | 0.02 | 0.17 |

**Table B.5 Firm Performance and Resource Allocation**

The table reports the estimates following specifications in Table 5. The dependent variable is the two-quarter forward return on assets (ROA) of firm $f$ in columns (1)-(2), and the one-year forward ROA of firm $f$ in columns (3)-(4). All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| **Dep. Variable** | **Two quarters forward *ROA*** | | **Four quarters forward *ROA*** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | SOE | Non-SOE | SOE | Non-SOE |
| $Treated_{fq} \times \Delta cash_f$ | 0.066* | 0.017 | 0.090 | 0.008 |
| | (0.04) | (0.02) | (0.10) | (0.02) |
| $Treated_{fq}$ | 0.120 | 0.183*** | -0.121 | 0.248*** |
| | (0.08) | (0.02) | (0.16) | (0.07) |
| Controls | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y |
| N | 21,290 | 33,939 | 19,857 | 30,988 |
| $R^2$ | 0.58 | 0.56 | 0.63 | 0.60 |

**Table B.6 Resource Allocation upon Adverse Shocks—SOEs and Non-SOEs**
The table reports the estimates of:

$$Ratio\ of\ guarantees\ deals_{fq} = \alpha_f + \alpha_q + \alpha_{area,qoy} + \beta Treated_{fq} + \gamma' x_{fq} + \varepsilon_{fq}$$

The dependent variable is the percentage of the net guarantees deals that bring in funds of firm $f$ in quarter $q$. On the right-hand side, $Treated$ is a continuous treated variable equal to 1 in the four quarters after an adverse shock breaks out in a city where firms are located; $x$ is a vector of control variables as in Table 3. Columns (1)–(2) report results based on "carrot" events for SOEs and non-SOEs, respectively. Columns (3)–(4) report results based on "stick" events for SOEs and non-SOEs, respectively. All columns include firm fixed effects, quarter fixed effects, and the product of economic zones and quarter of the year fixed effects. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| **Guarantees / total deals** | Carrot Events | | Stick Events | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | SOE | Non-SOE | SOE | Non-SOE |
| $Treated_{fq}$ | 5.580*** | -0.038 | -7.394** | -1.076 |
| | (1.90) | (2.23) | (3.32) | (7.39) |
| Controls | Y | Y | Y | Y |
| Quarter f.e. | Y | Y | Y | Y |
| Firm f.e. | Y | Y | Y | Y |
| Area × QoY f.e. | Y | Y | Y | Y |
| N | 15,932 | 29,693 | 8,024 | 8,501 |
| $R^2$ | 0.33 | 0.29 | 0.28 | 0.29 |

**Table B.7 Resource Allocation at Adverse Shocks—More Events**

The table reports the estimates following specifications (1)-(2) in Table 3. Additional social unrest events are included. In total, there are 25 events including peasants protests, labor riots, religion repression, subnational predominance, and social economic protests. All variables are defined in detail in Appendix A. Standard errors, reported in parentheses, are clustered at the city and quarter levels. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| Panel A | | |
|---|---|---|
| **Net flow / total assets** | Carrot Events | |
| | (1) | (2) |
| | SOE | Non-SOE |
| $Treated_{fq}$ | 0.585** | 0.164 |
| | (0.25) | (0.21) |
| Controls | Y | Y |
| Quarter f.e. | Y | Y |
| Firm f.e. | Y | Y |
| Area × QoY f.e. | Y | Y |
| N | 20,396 | 35,298 |
| $R^2$ | 0.17 | 0.17 |

| Panel B | | |
|---|---|---|
| **Political deals / total deals** | Carrot Events | |
| | (1) | (2) |
| | SOE | Non-SOE |
| $Treated_{fq}$ | 5.003** | 0.317 |
| | (2.16) | (1.23) |
| Controls | Y | Y |
| Quarter f.e. | Y | Y |
| Firm f.e. | Y | Y |
| Area × QoY f.e. | Y | Y |
| N | 20,396 | 35,298 |
| $R^2$ | 0.25 | 0.22 |

# Chapter 3

## Can Technology Undermine Macroprudential Regulation?

## Evidence from Peer-to-Peer Credit in China

**Abstract**

We study whether and to what extent peer-to-peer (P2P) credit helps circumvent loan-to-value (LTV) caps, a key macroprudential tool to contain household leverage. We exploit the tightening of mortgage LTV caps in a number of cities in China in 2013 as our testing ground, in a difference-in-differences setting, and we base our tests on a novel, hand-collected database covering all lending transactions at RenrenDai, a leading Chinese P2P credit platform. P2P loans increase at the cities affected by the LTV cap tightening relative to the control cities, consistent with borrowers tapping P2P credit to circumvent the regulation. The granularity of our data allows us to separate credit demand from credit supply effects, with a fixed effects strategy. Our results also indicate that P2P lenders do not adjust their pricing and screening to the influx of new borrowers after 2013, despite the fact that their loans ex post have higher delinquency and default rates. Symmetric effects are associated with a loosening of mortgage LTV caps in 2015. Our test provides empirical evidence on the capacity of P2P credit to undermine LTV caps. More broadly, our analysis informs the debate on the challenges posed by the interaction between FinTech and credit regulation.

JEL codes: G23, G01, G28.

Keywords: peer-to-peer credit, household leverage, macroprudential regulation, loan-to-value caps.

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## 3.1. Introduction

The global crisis of 2007-2009 has alerted economists and regulators about the risks that excessive household leverage poses to the financial system.[62] The debate on how best to contain household debt has focused on macroprudential tools; among them, much emphasis has been placed on loan-to-value (LTV) caps, which prevent borrowing beyond a certain fraction of the value of the assets to be purchased with the loan.[63] LTV caps typically target traditional financial intermediaries, such as banks. That, however, might be too narrow if households have access to alternative, lightly regulated credit channels that allow them to circumvent limits on borrowing from regulated lenders. In this paper, we study one emerging – and so far neglected – such channel: peer-to-peer (P2P) credit.

By now rivaling traditional consumer loans in size and reach (Morse (2015)), P2P credit has experienced double-digit growth in developed economies such as the U.S., where lending volumes amounted to $77bn in 2015.[64] The fastest-growing P2P credit market, however, is China, which is also estimated to be the largest in the world (Deer, Mi, and Yuxin (2015)), with volumes totaling over $90bn (RMB 600bn) as of June 2016, and corresponding to about 20% of consumption loans to households provided by traditional banks.[65]

A P2P credit company acts as a "broker," offering an online platform that brings together borrowers and prospective lenders. P2P credit can be a channel to elude LTV caps, because it provides borrowers with: (i) a form of anonymity, since P2P platforms typically receive much less regulatory scrutiny than banks, and (ii) access to an unprecedentedly large potential funding pool, in comparison to traditional non-bank credit sources such as family and acquaintances, payday

[62] There is a vast theoretical and empirical literature on the impact of (household) leverage on the 2007-2009 crisis, too large to sum up here. Part of the theory contributions stress the role of collateralized lending, with arguments based on Kiyotaki and Moore's (1997) seminal work, see e.g. Geanakoplos (2010), Gorton and Ordoñez (2014). Hall (2011) and Guerrieri and Lorenzoni (2017) show that high debt levels can exacerbate the downturn of the economic cycle even in the absence of collateralized lending. In several studies Adelino, Schoar, and Severino (2012, 2016), Mian and Sufi (2009, 2011), Mian, Sufi, and Trebbi (2015) document the relationship between U.S. household leverage and the severity of the 2007-2009 crisis. Bordo (2008), Claessens, Kose, and Terrones (2012), and Schularick and Taylor (2012) find that crises are typically preceded by periods of rapid credit growth. Mian and Sufi (2010) and Di Maggio and Kermani (2017) document the real economy disruptions associated with high household leverage.

[63] See for instance Allen and Carletti (2011), Crowe, Dell'Arriccia, Igan, and Rabanal (2011), Hanson, Kashyap, and Stein (2011), Claessens (2015), Jácome and Mitra (2015).

[64] E. Robinson, "As money pours into peer-to-peer lending, some see bubble brewing", *Bloomberg*, May 15, 2015.

[65] G. Wildau, "Chinese P2P lending regulations target hucksters and risk-takers", *Financial Times*, August 24, 2016.

lenders, credit cards, etc. We aim to assess to what extent the availability of P2P credit poses a vulnerability to LTV-based policies and contributes to fueling household debt creation.

Taking this question to the data confronts us with two empirical challenges. First, we are interested in gauging the capacity of P2P credit *supply* to undermine LTV caps. But the equilibrium in the market for loans also depends on credit *demand*; and separating demand and supply is difficult, because the econometrician only observes lending outcomes ex post. An increase in P2P loans, for instance, could be due to inefficient lending induced by excess credit supply, but just as well to improved economic prospects raising credit demand. Since Koopmans (1949), the approach to identify supply is to trace it out with demand shifts. Thus, we require a shock to the demand for P2P lending, which does not separately affect its supply.

Second, in order to trace out credit supply with demand shocks, we must control for potential supply-side drivers, mainly in the form of unobserved heterogeneity among P2P lenders. For instance, lenders may differ in terms of their proximate knowledge, due to their expertise (Morse (2015)) or their ability to harness information from social circles for screening and monitoring (Freedman and Jin (2014), Lin, Prabhala, and Viswanathan (2013)). To the extent that lenders' characteristics such as these can vary with the exposure of their borrowers to a demand shock, the resulting simultaneous changes in credit demand and supply can confound the interpretation of any test. Thus, while we study the effects of a change in P2P credit demand, we want to be able to hold the P2P lending supply curve fixed.

In sum: To design our test, we need a shock to the demand for P2P credit, as well as a way to hold P2P credit supply fixed. The setting of our analysis allows us to address the first challenge. The structure of our data helps us address the second one.

We study P2P credit around a regulatory change in the Chinese real estate market, which takes place in November 2013. The city governments of a number of large Chinese cities impose a 16.7% increase in the minimum down-payment required to obtain a mortgage for the purchase of a second home, raising it from 60% to 70% of the property's value. The objective is to slow down the growth in real estate prices, following a policy impulse in this direction by the Chinese central government.[66] Anecdotal evidence, however, suggests that real estate investors circumvent

[66] "Shanghai Raises Home Down-Payment Requirement as Prices Jump", *Bloomberg*, November 8, 2013, and "China's Nanjing, Hangzhou Raise 2nd Home Down Payments", *Bloomberg*, November 27, 2013.

the new requirements, borrowing via P2P credit platforms to meet the increased down-payment.[67] In other words, with a relatively small P2P loan, households are able to increase their leverage by the (much larger) full amount of the mortgage. Importantly, the regulatory change creates a positive shock to P2P credit demand, thus addressing our first empirical challenge.[68]

We exploit this policy intervention in a difference-in-differences setting, studying changes in P2P credit around this episode, for affected and un-affected cities. We assemble a novel, hand-collected database containing all loan applications and credit outcomes for a leading Chinese P2P credit platform, RenrenDai (人人贷). Our database contains all the transactions executed within the platform, and it matches each borrower with her lenders.

Our results are consistent with P2P lending providing an unregulated source of credit with the potential to undermine LTV caps. In the analysis, our baseline effects, however, are already visible in Figure 1, which plots loan application volumes at RenrenDai, for "treated" and "control" cities, around the last quarter of 2013. The lines corresponding to treated and control cities closely overlap over the entire two-year period preceding the regulatory change. Following the last quarter of 2013, however, loan applications in the treated cities increase sharply relative to the control cities, consistent with an influx of applications to help meet the higher down-payments. While RenrenDai loan applications grow in both groups, due to the boom in P2P credit in China during our sample period, in the first six months of 2014 applications in the treated cities grow by 50%, as opposed to only 16% in the control cities. These findings are in line with our key predictions, and provide a first piece of evidence consistent with P2P credit being instrumental to circumventing the regulatory LTV cap.

Our subsequent tests validate this visual check, and strengthen the case for a causal interpretation. City- and borrower-lender level regressions confirm the evidence from Figure 1. In particular, we leverage the depth of our data with the borrower-lender level regressions, which allow us to trace the impact of the P2P lending demand shock controlling for lender × date fixed effects. These estimates compare the P2P credit received by different borrowers from *the same*

[67] D. Weinland, and Y.Yang, "China to Crack Down on P2P Lenders," *Financial Times*, March 14, 2016.

[68] The cities imposing the increase in down-payment requirements are Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Shanghai, Shenyang, Shenzhen, and Wuhan. Most cities increase mortgage down-payment requirements in November, with the only exception of Beijing, which increases them in March. For that reason, the visual analysis of Figure 1 is focused on the last quarter of 2013. In the tests reported in the subsequent tables, however, Beijing is considered a treated city starting in the second quarter of 2013.

*lender at the same point in time*, thus holding credit supply capacity fixed and addressing our second empirical challenge.

Our estimates imply that the increase in P2P loans we observe accounts for about 10% of the increase in down-payment requirements for large cities like Shanghai or Beijing, and over 35% in smaller cities like Changsha, Shenyang, or Wuhan. Given that RenrenDai, though an important market player, is but one of a large number of P2P platforms active in China, and that borrowers may be able to obtain credit on multiple platforms at the same time (Aggarwal and Stein (2016)), this estimate provides a lower bound on the importance of P2P lending as a channel to circumvent regulatory LTV caps. Consistent with this view, the regulatory intervention itself appears largely ineffective: house price growth at the treated cities does not slow down relative to the control cities after November 2013.

Our results also suggest that P2P lenders fail to adjust their screening and loan pricing decisions in the face of the influx of borrowers seeking to circumvent down-payment requirements. We find little evidence of changes in the credit scores and rates of on-site verification for borrowers who obtain a loan after the 2013 episode (tighter screening would imply increases for both), nor do we observe any significant changes in loan yields or maturities. This is in spite of the fact that default rates increase, driven primarily by "new" borrowers, who come to RenrenDai only after November 2013. These results suggest that lenders have an "inflexible" lending technology, and do not adjust their lending decisions, even though they are making loans that turn out to be riskier.

We validate this analysis studying a symmetric change in LTV caps, which takes place in September 2015. Starting from that month, all the city governments in China, with the exceptions of Beijing, Guangzhou, Sanya, Shanghai, and Shenzhen, impose a 16.7% reduction in minimum down-payment requirements, now as well for first home purchases (from 30% to 25% of the property's value). In this case the demand for P2P lending at the treated cities decreases relative to the controls, reversing the effects observed around the 2013 episode.

Our findings make three contributions. First, to the best of our knowledge, this is the first paper empirically studying the interaction between new financial technologies and credit regulation. One could view FinTech, and in particular P2P lending, as a form of shadow banking. Regulatory arbitrage has long been considered one of the main drivers of the growth of shadow banking. Interestingly, however, a large part of the literature has focused on the elusion of regulatory constraints by financial intermediaries such as banks, i.e. on the side of credit *supply*

(Adrian and Ashcroft (2012), Plantin (2014)). Our results are consistent with the view that regulatory arbitrage on the side of credit *demand* (facilitated by the presence of the more lightly regulated P2P channel) can also be economically very relevant.

Second, our paper contributes to the literature on the drivers of household leverage. Financial (il)literacy (Lusardi and Tufano (2009)), real estate prices (Mian and Sufi (2011), Crowe and Ramcharan (2013)), and import competition (Barrot, Loualiche, Plosser, and Sauvagnat (2017)) have been found to be important factors behind household debt. Our findings suggest a new, and so far neglected factor: The development of financial technology and the disintermediation of financial services.

Third, our test speaks to the ongoing debate on the systemic impact of household leverage, and on the design of policies to contain it. Much of the literature has focused on U.S. data, and two views prevail. One view focuses on credit supply, and blames financial innovation and incentives in the financial sector for the buildup of mortgage debt leading to the 2007-2009 crisis (Mian and Sufi (2009), Claessens, Dell'Arriccia, Igan, and Laeven (2010)). A second view focuses on credit demand, on the grounds that household leverage growth encompassed not only lower-income borrowers, but also the middle-class ((Adelino, Schoar, and Severino (2012, 2016), Foote, Gerardi, and Willen (2012), Foote, Loewenstein, and Willen (2016), Albanesi, De Giorgi, and Nosal (2017)). Our findings present fresh evidence from a different context – China – and time period – 2010-2016 – and highlight the role of both credit demand (to meet the down-payment requirements) and credit supply (from P2P lending). They also point to a vulnerability of LTV caps, a tool on which much of the debate on macroprudential regulation has focused (Allen and Carletti (2011), Crowe, Dell'Arriccia, Igan, and Rabanal (2011), Claessens (2015), Jácome and Mitra (2015)). A potential solution would be to monitor other indicators than LTV (for example, debt-to-income), as well as the borrowers' overall indebtedness. The risk, however, is to throw out the baby with the bathwater, losing the flexibility that makes P2P credit successful in the first place.

The remainder of the paper is organized as follows. Section II lays out our empirical predictions with the aid of a simple model. Section III presents our data and discusses our identification strategy. Section IV reports our baseline findings on changes in P2P lending volumes around the 2013 increase in down-payment requirements, and Section V on changes in loan pricing and screening in the P2P lending market. Section VI presents similar tests around the 2015

decrease in down-payment requirements. Section VII discusses the policy implications of our findings. Section VIII concludes.

## 3.2. Predicted impact of the change in LTV caps

We analyze the impact of the unregulated P2P credit channel on the effects of changes in regulatory LTV caps (mortgage down-payment requirements), using a framework that builds on Holmstrom and Tirole's (1997) workhorse fixed investment model. The rise in down-payment requirements to borrow from traditional lenders is analogous to a "collateral squeeze," which curbs credit in Holmstrom and Tirole's model. We show that the availability of P2P lending allows borrowers to circumvent the tightened LTV cap, sterilizing its effects such that the levels of new credit, aggregate interest costs, and defaults are not reduced. These results allow us to formulate the key empirical predictions for our proposed test.

First, we consider an economy populated by households (borrowers) and competitive traditional, regulated lenders ("banks"). At a later stage, we introduce unregulated ("P2P") lenders. Households seek credit to acquire real estate, and when borrowing from a bank they are subject to an endogenous down-payment requirement $\bar{A}$ (derived below), plus an additional margin $\delta$ imposed by the regulator. We model the 2013 tightening of the LTV cap as an increase in $\delta$, and study its effects on the total amount of debt promised interest payments level, and default rates in the economy.

As in Holmstrom and Tirole (1997), borrowers are subject to moral hazard. They are able to generate future cash flows $Y \in \{0, y\}$, which they will use to pay back their loans. The probability of positive cash flows $\Pr(Y = y) = p$ takes values in $\{p_L, p_H\}$, with $p_H - p_L = \Delta_p > 0$. A borrower needs to exert "effort" to raise the success probability to $p_H$, and the borrower's utility from not exerting effort is $B$.

Each would-be borrower has assets-in-place $A$, representing to her ability to meet a down-payment requirement, and needs to borrow $I - A$ to make her real estate purchase. If the borrower does not default, she splits her cash flow with the bank such that $y = d_b + d_l$. If the borrower

defaults, the bank recovers a value $R < I$ (e.g. as the result of a foreclosure process).[69] $R$ is exogenously given.[70] Intuitively, the bank wants to induce $p_H$, providing the borrower with an incentive contract.

The participation constraint for the bank is $p_H d_l + (1 - p_H)R \geq I - A$, i.e. the bank must expect a larger payoff if it makes the loan than if it holds on to its cash $I - A$. This implies:

$$d_l \geq \frac{1}{p_H}[I - A - (1 - p_H)R]. \quad (1)$$

Since banks are competitive, (1) holds with equality. The incentive compatibility constraint for the borrower is $p_H d_b \geq p_L d_b + B$, i.e. the borrower must prefer to exert effort, so that:

$$d_b \geq B/\Delta_p. \quad (2)$$

Combining (1) and (2) with the resource constraint $y = d_l + d_b$, and assuming $y \geq B/\Delta_p + d_l$, we have the following condition for the bank to make a loan:

$$A \geq \bar{A} = I - p_H \left[y - \frac{B}{\Delta_p} + (1 - p_H)\frac{R}{p_h}\right] \quad (3)$$

Expression (3) implies that only borrowers with sufficiently high assets-in-place (i.e. able to meet the down-payment requirements) obtain credit.

To analyze the equilibrium of the credit market in this setting, suppose that there is a continuum of borrowers indexed by their assets-in-place $A$, distributed according to a cdf $G(A)$. The total amount of credit in equilibrium is then $I[1 - G(\bar{A})]$. Denoting the bank's required interest rate by $i_l$, by definition $d_l = (I - \bar{A})(1 + i_l)$, so that from expression (1) we have:

$$i_l = \frac{1}{p_H}\left[1 - \frac{R(1-p_H)}{I-\bar{A}}\right] - 1 \quad (4)$$

and the aggregate interest owed in the economy is $i_l[1 - G(\bar{A})]$.

Consider now the effects of an "LTV cap tightening," in which the regulator mandates that borrowers possess an additional $\delta > 0$ over and above $\bar{A}$, in the form of a mandatory minimum down-payment requirement (in the language of Holmstrom and Tirole (1997), this is equivalent to a "collateral squeeze"). The resulting total amount of lending is $I[1 - G(\bar{A} + \delta)]$. As the function

[69] This ingredient is not present in Holmstrom and Tirole's (1997) original formulation. We introduce it for two reasons. First, it simplifies the exposition in our setting. Second, it better reflects the reality of real estate mortgages, where the lending bank's recovery can correspond to the value of the property, following the foreclosure.

[70] One could think of an extension of this analysis where the value of $R$ is determined in equilibrium. Household leverage could then affect the value of collateral $R$ and impose fire sale externalities, similar to the arguments of Shleifer and Vishny (1992). Such externalities could provide a rationale for the regulator's intervention (i.e., raising $\delta$ with the aim of limiting credit growth).

$G(\cdot)$ is monotone increasing, this is less than $I[1 - G(\bar{A})]$, i.e. the new LTV cap curbs the level of debt in the economy. Because the bank has a smaller exposure to each borrower, moreover, interest rates decrease via expression (4), and the aggregate interest costs are reduced. Finally, because there are fewer borrowers, there are also fewer aggregate defaults $(1 - p_H)[1 - G(\bar{A} + \delta)]$. In sum: tightening the LTV cap reduces new credit, as well as aggregate interest payments and defaults.

What happens when unregulated P2P lenders are introduced? The P2P lenders are assumed to be competitive, as well as "small," in the sense that they cannot lend more than $\delta$ to any borrower. These assumptions mimic the features of the P2P lending market in China (Deer, Mi, and Yuxin (2015)). A simple strategy for a borrower who fails to obtain credit from the bank because her assets-in-place are below $\bar{A} + \delta$, then, is to borrow $\bar{A} + \delta - A$ from the P2P lenders, so as to be able to make the full $\bar{A} + \delta$ down-payment. We study whether this strategy can be sustained in equilibrium, and its implications.

There are two main differences between P2P lenders and banks. First, the P2P lenders are not collateralized, i.e. in the event of default their payoff is equal to $0$. Second, they do not condition their lending decisions on the borrower's assets-in-place, but simply take her default risk as given. This implies that the participation constraint for the P2P lenders is:

$$p_H d_{P2P} \geq \bar{A} + \delta - A, \tag{5}$$

where the P2P lender payoff is scaled by $p_H$ because the borrower also receives credit from the bank, which provides the incentive to exert effort.[71]

For large enough $y$, this will be true in equilibrium. Because the P2P lenders are competitive, the constraint (5) holds with equality, and $d_{P2P} = (\bar{A} + \delta - A)/p_H$. The participation constraint for the bank (1) is now modified as $d_l = \frac{1}{p_H}[I - (\bar{A} + \delta) - (1 - p_H)R]$, and the incentive constraint for borrowers remains $d_b \geq B/\Delta_p$. The minimum level of assets-in-place $\bar{A}$ required to obtain credit from the bank is again pinned down by the resource constraint $y = d_l + d_{P2P} + d_b$, and because the $\bar{A} + \delta$ terms in $d_l$ and $d_{P2P}$ cancel out, $\bar{A}$ is again given by expression

[71] As we verify below, in equilibrium borrowers turn to the bank first, and only if they do not have sufficient assets-in-place $A$ they also borrow from the P2P lenders. This allows the P2P lenders to "free ride" on the incentives provided by the bank. P2P credit is still more expensive, because the recovery under default is 0 (while the bank recovers $R$). Alternatively, one could assume that the probability of default remain "low" ($p_L$) for P2P loans, without changing the main conclusions.

(3). In other words, regardless of the size of the increase in the down-payment requirement $\delta$, an identical mass $1 - G(\bar{A})$ of borrowers obtains credit, and the level of debt in the economy is unchanged. Similarly, the expected number of defaults remains $(1 - p_H)[1 - G(\bar{A})]$. Also similarly, aggregate promised interest payments do not change. The interest rate demanded by the P2P lenders, implied by (6), is: $i_{P2P} = \frac{1}{p_H} - 1$. This is larger than $i_l$, because of the recovery value $R$. However, the banks make loans with a lower LTV ratio, and as a result demand lower interest. These two effects balance each other exactly. Aggregate interest costs, in other words, are equal to:

$$\underbrace{i_l \times [1 - G(\bar{A} + \delta)]}_{\text{To the bank}} + \underbrace{i_{P2P} \times [G(\bar{A} + \delta) - G(\bar{A})]}_{\text{To P2P lenders}}. \quad (6)$$

Because $\frac{\partial}{\partial \delta}(d_l + d_{P2P}) = 0$, aggregate interest payments are unchanged. In sum: the objective of the tightened LTV cap is to curb new credit, reducing aggregate interest costs and aggregate defaults. The availability of P2P credit, however, sterilizes the cap, leaving new credit, aggregate defaults, and aggregate promised interest payments unchanged.

This analysis allows us to formulate our key empirical predictions. The 2013 increase in down-payment requirements corresponds to an increase in $\delta$. Changes in $\delta$ do not affect the overall level of credit in the economy, but simply shift demand into and out of P2P lending. Therefore: Following the 2013 increase in down-payment requirements, we will observe a larger volume of new P2P loans in cities that raise mortgage down-payment requirements (treatment group) than in other cities (control group).[72]

## 3.3. Data and identification

*A. Data*

We base our analysis on a large, loan- and loan application-level database from a leading Chinese online P2P credit platform, RenrenDai. RenrenDai was launched in 2010, and quickly developed into one of the main players in the Chinese P2P credit sector, with cumulative turnover of RMB

[72] This model provides a simple framework to form expectations on the impact of P2P lending on the effectiveness of the 2013 (and, in a further test described below, 2015) policy intervention in the mortgage markets. A byproduct of its simplicity is that aggregate defaults remain unchanged at $(1 - p_H)[1 - G(\bar{A})]$, because individual borrower default risk is constant. A more flexible model might generate increasing default rates as borrowers turn to P2P lending (consistent with the evidence we discuss in section V). We feel that such a model is beyond the scope of our study, as our focus is mainly empirical.

25bn ($3.7bn, as of February 2017) and over 3 million registered accounts (2016). Among the over 2,000 Chinese P2P credit platforms active as of December 2016, RenrenDai ranks, by turnover, in the top 1%.[73]

Our database spans the period from October 2010, when RenrenDai first opens to the public, until November 2016. In total, the data contain 909,649 loan applications, made by 703,028 individual borrowers, and involving 277,761 lenders.

Table 1 reports summary statistics for out data, over a window of 37 (–18, +18) months around the 2013 mortgage LTV cap tightening. The average loan has a size of RMB 59,674 ($8,730), with an annualized interest rate of 12.5% and duration 27 months. The average RenrenDai borrower has a pre-tax monthly income of RMB 11,000 ($1,610), or about RMB 130,000 ($19,018) yearly. Based on data from the China Household Finance Survey, the mean after-tax yearly income for Chinese individuals with outstanding debt, living in non-rural areas in the provinces where RenrenDai is active, is RMB 74,000 ($10,240).[74] With an average income tax rate of about 40%,[75] therefore, RenrenDai borrowers appear in line with the population average. The loan face value is typically about 40% of the borrower's annual income. In comparison, Morse (2015) reports average interest rates of about 14%, loan duration of 41 months, and loan face value of 20.5% of the borrower's annual income. We thus observe higher loan-to-income ratios and shorter durations, but similar interest rates as in the U.S. There is also sparse information on the purpose of the loans; the most common purposes are "Short Term Liquidity Needs" (48%), "Consumption/General" (25.7%), and "Entrepreneurship" (8.8%). In their survey of P2P lending in China, Deer, Mi, and Yuxin (2016) find that 51% of survey participants claim to use P2P lending to "accumulate credit worthiness," consistent with borrowing to meet a down-payment requirement, as in the 2013 episode on which we focus. The data also report each borrower's credit score, based on RenrenDai's internal scoring system. There appears to be relatively little variation in credit scores: the average score is 172 (with standard deviation about 30), the median is 180, and the maximum is 181.

[73] In 2017, RenrenDai accounts for 1% of total p2p market share in China according to industrial level data from wdzj.com.

[74] The China Household Finance Survey is administered by the Southwestern University of Finance and Economics. The data are based on the 2011 wave of the survey (the only one available at the time of writing).

[75] Income taxes are progressive in China (cf. e.g. https://www.ecovis.com/focus-china/individual-income-tax-iit-china-ground-rules/). The 40% average tax rate is based on a back-of-the-envelope calculation for an individual with a pre-tax income of RMB 130,000 as in our data.

For each borrower in our data, in addition to her income level we are able to observe a number of characteristics, including demographics such as gender, age, city of residence, etc. Additional data items are disclosed by the borrowers on a voluntary basis, such as education, home ownership, and whether or not they have a mortgage. Average borrower age is about 38 years; around 50% of borrowers have a college degree, and 64% are male. Unlike in the U.S. (Balyuk (2016)), the median RenrenDai borrower is a home owner, and about 69% of borrowers who are home owners have a mortgage. Disclosing more information allows the borrower to obtain a higher credit score on RenrenDai's internal rating system, so that borrowers have an incentive to greater disclosure. In our data, 99.86% of all successful loan applications are associated with borrowers who disclose at least some of these non-mandatory items. The median borrower in our data only obtains one loan; there are, however, repeat borrowers, with up to 148 loans in their history on RenrenDai.

Similar to studies based on U.S. P2P credit data (e.g. Balyuk (2016), Morse (2015)), we are not able to directly observe lender characteristics, but we can characterize them by looking at the features of the lenders' loan portfolios. In Table 1.C, we report the characteristics of the average lender on a given loan (the mean number of lenders per loan is 45; median: 30). On average, lenders hold a portfolio of 235 loans, with a total face value of RMB 387,978 ($58,197).[76] Portfolios are generally diversified, with an HHI concentration index of 0.007 on average, and the average lender on a given loan has an experience on RenrenDai of about 7 months. Finally, lenders can choose to make their loans directly to borrowers, or delegate the allocation of their funds across different loans to Uplan (U 计划), an algorithm that matches lenders to borrowers mostly based on returns and maturity preference parameters set by the lender. Around 70% of all loans are made via Uplan.

*B. Identification approach*

The structure of our data helps us address the identification challenges discussed in the introduction. In particular, to each lender on the RenrenDai platform is associated a unique ID code, and the typical lender invests in multiple loans at the same time. This allows us to control for unobserved lender heterogeneity and hold credit supply fixed with a fixed effects strategy.

[76] The average lender's portfolio size is about three times the annual income of the average borrower in our data. This suggests that RenrenDai lenders are relatively wealthy, and may have some degree of financial sophistication.

Intuitively, our test compares two loans, made by the *same P2P lender, at the same point in time*, to two different borrowers, Fang and Wei. Fang is exposed to the increase in down-payment requirements; Wei is not. Because the P2P lender is the same on both loans, any factor affecting the *supply* of credit from the lender, related e.g. to her lending capacity, market strategy, technology etc. can thus be ruled out, allowing us to focus on the difference in credit *demand* between borrowers Fang and Wei. Operationally, we exploit the wealth of information at our disposal by running our tests on loan-lender level data, with lender × date fixed effects.[77]

We analyze changes in P2P loans, comparing affected and un-affected real estate markets around the 2013 and 2015 changes in minimum mortgage down-payment requirements described above. The baseline test takes the form of a classic difference-in-differences regression:

$$L_{blt} = \alpha + \beta Treated_{bt} + \gamma Post_t + \delta(Treated_{bt} \times Post_t) + \mu' x_{blt} + \varepsilon_{blt} \qquad (7)$$

where $L_{blt}$ denotes a loan associated with borrower $b$ and lender $l$ at time $t$. $Treated$ is an indicator variable equal to 1 if the borrower is located in one of the cities affected by the change in minimum down-payment requirements. $Post$ is an indicator variable equal to 1 in the period subsequent to the change in down-payment requirements. To be immune to the Bertrand, Duflo, and Mullainathan (2004) critique of standard errors in difference-in-differences tests, we collapse the data and take averages over two periods, before and after the change in down-payment requirements, and then take first differences, estimating:

$$\Delta L_{bl} = \alpha + \delta Treated_{bt} + \mu' \Delta x_{bl} + \eta_{bl} \qquad (7')$$

where $\Delta L$ denotes the change in loan applications around the regulation change, associated with borrower $b$ and lender $l$.

Given the features of the data at our disposal, we are going to be able to estimate model (7)-(7') on different levels of granularity, allowing to control for alternative potential confounding factors. In the simplest specification, we aggregate equation (7') to the city-date level, i.e. studying the behavior of all loans (applications) in a given city at a given point in time around each change in down-payment requirements.

In a second specification, we estimate model (7) on the individual loan-lender level, i.e. where each observation corresponds to a given loan, associated with a given lender and borrower.

[77] This approach is close in spirit to the fixed effects strategies adopted in the literature on bank liquidity shocks (e.g. Khwaja and Mian (2008); Schnabl (2012)). Note, however, that studies in that literature typically control for *borrower* fixed effects, as their objective is to hold credit demand constant, to examine the effects of credit supply shocks. In our case, we want to hold credit supply constant, and thus control for *lender* fixed effects.

This specification allows us to exploit the full depth of our data, and hold the credit supply curve fixed, saturating the model with lender × date fixed effects as discussed (this is equivalent to including lender fixed effects in equation (7')).

*C. Comparison of treatment and control groups prior to November 2013*

Our main tests are focused on the 2013 increase in down-payment requirements. The cities that experience it include four of the ten largest cities in China (Beijing, Guangzhou, Shanghai, and Shenzhen) and overall make up about 9% of the population of urban China in our sample on average.[78] In addition, the treatment affects both "tier-1" (Beijing, Guangzhou, Shanghai, and Shenzhen) and "tier-2" (Changsha, Hangzhou, Nanjing, Shenyang, and Wuhan) cities. We take all other Chinese cities with active borrowers on RenrenDai and population over 5 million as our control group.

In Table 2, we compare the loans associated with the treatment and control cities along observable dimensions, prior to November 2013. Panel A focuses on borrowers. Borrowers from treated and control cities do not exhibit significant differences in terms of monthly income (RMB 11,216 and 11,872 on average), age (about 39 for both groups), gender (59% and 57% males), or the number of loan applications since registering on RenrenDai (1.5 and 2). Treated borrowers are modestly more likely to have a college degree (50.6% have one, compared to 45.1% for the control group; t-stat: 1.695), and less likely to be home owners (18%, compared to 27% for the control group; t-stat: -2.04).[79] Panel B compares lenders across the two groups. In terms of portfolio size, concentration, experience, and participation to Uplan, there are no significant differences between the treated and control groups, in statistical as well as economic terms. Finally, in Panel C the treated and control cities are compared in terms of macroeconomic variables. We detect no significant differences along the dimensions of per capita GDP (level and growth), population (level and growth), household net debt to income, real wages, and RenrenDai penetration rates.

In sum, we do not observe large differences along observable dimensions between the treatment and control groups prior to the increase in down-payment requirements of November 2013. That confirms the intuition from Figure 1, which shows parallel trends in P2P lending in the

[78] Communiqué of the National Bureau of Statistics of the People's Republic of China on the Major Figures of the 2010 Population Census. We restrict the sample to cities with an average population of at least 5 million during our sample period (all the results are robust to including smaller cities).

[79] These values are based on observations prior to November 2013, explaining the difference from the average home ownership rates in Table 1, which are based on the entire sample.

two groups in the pre-down-payment increase period, and validates the difference-in-differences setting for our test.

## 3.4. Baseline tests

We first run a set of preliminary regressions on city-level data. We estimate model (7') by time-averaging, collapsing the data, and taking first differences, as described above, to control for serially correlated standard errors (Bertrand, Duflo, and Mullainathan (2004)).

The results are reported in Table 3. The estimates in Table 3.A support the evidence from Figure 1, as well as the arguments illustrated in Section II. They imply that, following the 2013 rise in down-payment requirements, the RMB volume of P2P loans in cities affected by the rise increase by 26.4% on an annual basis (specification (2)), which appears economically substantial.

Separate tests show that house price growth does not slow down in the treated cities – despite the fact that was precisely the aim of the regulatory intervention. The estimates reported in Table 3.B have specification analogous to Table 3.A, where the dependent variable is the quarterly change in house prices in a given city. The implied effects are near zero, and may be positive or negative depending on the specification. In specification (3) we actually observe a positive and significant coefficient on the $Treated$ indicator, implying an increase in house price growth in the treated cities relative to the control cities. Economically, however, the difference is modest, at 1.6 percentage points per quarter. In sum, it appears that the rise in down-payment requirements was largely ineffective in slowing down house price growth at the treated cities.

The evidence from these preliminary tests is consistent with the notion that borrowers use P2P lending to circumvent the increase in down-payment requirements. A rise in P2P loans, however, can be in general the result of a combination of shifts of the credit demand and credit supply curves. For instance, a faster development of P2P lending, or a greater popularity of P2P as a form of investment at the treated cities, might generate similar effects as the ones we observe in Table 3. To control for credit supply side effects, we estimate model (1) on data matching individual lenders and borrowers, controlling for lender × date fixed effects. As discussed above, this allows us to hold credit supply fixed, and isolate the effect of a shock to credit demand.

The estimates are reported in Table 4. Specifications (1)-(3) include lender × date fixed effects; specification (4) reports the corresponding estimates without them. Overall, the estimates

are in line with those of Table 3, and consistent with an increase in P2P lending demand to circumvent the down-payment requirement increase. Economically, the effects are also meaningful: they imply a 2.5% monthly increase in P2P lending at the treated cities, or 30% on an annual basis. Compared to the average loan size of about RMB 60,000 (about $8,700), this corresponds to a RMB 18,000 increase.

The value of a medium-size apartment (70 sq. meters) in 2013 in Shanghai, one of our treatment group cities, is RMB 1.8m (about $260,000), so that the increase we document accounts for 10% (= RMB 18,000 / RMB 180,000) of the 10-percentage point increase in down-payment requirements.[80] In tier-2 cities included in our treatment group, like Changsha, Shenyang, and Wuhan, the economic effect is even larger, corresponding to over 35% of the increase in down-payment requirements. Given that RenrenDai, though an important market player, is but one of a large number of P2P lending platforms active in China, and that borrowers may be able to obtain credit on multiple platforms at the same time (Aggarwal and Stein (2016)), these figures likely provide a lower bound on the importance of P2P lending as a channel to circumvent the new requirement.

We also separately analyze the intensive margin (borrowers already active on RenrenDai increase their borrowing) and the extensive margin (new borrowers turn to RenrenDai once down-payment requirements increase). To do so, we estimate two additional regressions, in columns (5) and (6). In column (5) (intensive margin), the sample is restricted to borrowers who are active on RenrenDai (have at least one loan) both before and after November 2013. In column (6) (extensive margin), the sample is restricted to borrowers who are active (have at least one loan) only before or only after 2013. The coefficient estimate on $Treated$ in the intensive margin regression is -0.001, indistinguishable from zero; the corresponding estimate in the extensive margin regression is 0.025 (t-stat: 2.08). The difference between the two coefficients is approximately equal to the estimated coefficients on $Treated$ in specifications (1)-(3), suggesting that the effect is driven by the *extensive* margin: in other words, the influx of new borrowers after the 2013 increase in down-payment requirements explains our baseline effect.

Further analysis based on borrower characteristics, reported in Table 5, provides a richer characterization of these findings. We document that the increase in P2P borrowing at the treated

[80] We obtain city-level data on house prices per square meter from the databank of China Index Academy, a leading real estate research organization in China.

cities is driven by loans to home owners (specifications (1)-(2)). This is consistent with the fact that the LTV cap tightening only affects second-home mortgages. The implied economic effects are also larger in this case, accounting for 12% of the required additional down-payment in tier-1 cities and over 50% in tier-2 cities. We also find that our baseline effect is driven by cities where house price growth over the 18-month period prior to November 2013 has been above the median.[81] These findings corroborate the link between the increase in P2P credit at the treated cities and the increase in down-payment requirements in November 2013, given that the new regulation only applies to second-home mortgages, and was aimed at overheating real estate markets.

Finally, in Table 6 we partition the sample based on lender characteristics: lending via Uplan/direct lending, experience, and portfolio size. Table 6.A shows that our effect is mainly associated with lenders who make loans as part of Uplan, which account for the majority of loans in our data (specifications (1)-(2)).[82] Moreover, borrowers in the treated cities receive financing from lenders regardless of the level experience: in both specifications (3) and (4), the coefficient on $Treated$ is positive and statistically significant. The estimated effect is, however, larger in magnitude for lenders with below-median experience.

Table 6.B distinguishes lenders based on their portfolio size.[83] Across lender portfolio size quartiles, the coefficients on $Treated$ are positive; but they are larger and statistically significant in the third and fourth quartiles, suggesting that the increase in P2P lending at the treated cities after November 2013 is driven primarily by larger lenders. To the extent that lenders with larger loan portfolios are likely financially more sophisticated, it appears that greater lender sophistication is no obstacle to the increased P2P lending.[84]

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[81] The estimates in Table 5 keep the control group observations fixed between the home-owner/non-home-owner and the above/below median house price growth subsamples. In this way, we consider increments in P2P lending that happen both on the extensive margin (more homeowners borrow from the platform in treated cities) and intensive margin (homeowners demand larger loans in treated cities).

[82] One concern is that Uplan algorithm might evolve from learning, and our identification strategies would be insufficient to control the credit supply effect. This concern can be alleviated for the reason that the algorithm needs time to learn, and it is less likely to change significantly during our relatively short period of focus.

[83] Because RenrenDai opens to the public in 2010, the range of lender experience (which we measure as months since making the first loan on RenrenDai) is limited. For that reason, we split the sample by lender experience at the median. In contrast, lender portfolio size has a much larger range, allowing us to split the sample by quartiles.

[84] At the same time, even lenders in the largest portfolio size quartile do not appear to have especially large amounts invested via RenrenDai. The average portfolio size in that quartile is about RMB 530,000 ($79,500), and the largest portfolio in our sample has size about RMB 4,100,000 ($615,000), indicating that we are looking at individual lenders (rather than e.g. institutions).

Taken together, these findings suggest that P2P lending supply responds to the credit demand generated by the 2013 increase in down-payment requirements as predicted by our discussion of Section II. P2P lenders are able to supply an economically substantial amount of credit, accounting for 12%-49% of the implied increase in borrowing as per our back-of-the-envelope calculation. The expansion of credit is driven by borrowers from cities that experience faster house prices growth, as well as by a broad range of lenders.[85]

## 3.5. Other loan features; loan performance

*A. Screening, pricing, and duration of loans*

P2P lenders may respond to an influx of loan applications following the 2013 down-payment requirement increase by adjusting lending volumes (which we analyzed in the previous Section), but also other contract features. We consider three central features of loan contracts: the degree of screening to which the borrower is subject, pricing, and duration.

Although the treated cities generate an abnormal amount of P2P borrowing after 2013, we find that P2P lenders do not appear to alter their screening in response. Our first measure of screening is on-site verification. Borrowers on RenrenDai self-declare their characteristics such as income, age, etc. In addition, they may also provide on-site verification, whereby an officer from RenrenDai verifies that the information provided is true, by visiting the borrowers at their stated address. If lenders respond to the influx of new borrowers by stepping up screening and tightening their lending standards, they may more frequently demand on-site verification in order to invest in a given loan. We should therefore expect higher rates of on-site verification among the loans made after the last quarter of 2013. We detect, however, no evidence of a change in on-site verification rates in Table 7 (in fact, we observe a slight, although statistically insignificant, decrease in specification (1)). Similarly, tighter screening predicts higher borrower credit scores on the loans. However, we also find little evidence of an increased borrower credit score (specifications (3)-

[85] Throughout our analysis we implicitly assume that borrowers use P2P funds to purchase a home in the city where they live. A possible concern is that borrowers in control cities borrow funds on RenrenDai to buy a house in a treated city. In principle, this possibility would make our control and treatment groups more alike, working against our test and suggesting that our estimates represent a lower bound of the effects of interest. In addition, every city in our treated group has home purchase restrictions in place that actually prevent residents from other cities to purchase a second home in the areas under their jurisdiction. For instance, only a registered resident in Shenzen is allowed to buy a second home in Shenzen, ruling out the possibility that a P2P borrower in, say, Chengdu (a city in our control group) may borrow on the platform to fulfil the down payment requirement set by another city.

(4)). Taken together, these results indicate that the lenders simply do not become more discriminating after November 2013.

In line with these findings, the pricing and duration of loan contracts issued after 2013 also do not change appreciably. We find no significant changes in yield spreads (specifications (5)-(6)), nor in duration (specifications (7)-(8)), after 2013. In sum, P2P lenders treat the influx of borrowers from the treated cities just like their old borrowers, and lend to them at conditions that are no different. This suggest that lenders make no adjustments to their lending terms following 2013. The interesting question is, of course, whether this can be rationalized ex post, for instance because the "new" loans perform similarly to the "old" ones.

*B. Loan performance*

We test for this possibility by looking at two measures of loan performance: delinquencies (the proportion of months during which the borrower is delinquent over the loan's life) and loan default rates. The sample size shrinks in this case, because of a truncation problem: for some ongoing loans, default may simply not have been declared yet.

The evidence, reported in Table 8.A, indicates a deteriorating loan performance at the treated cities following 2013. We observe an increase in delinquency rates by 0.90 percentage points (specification (1)) and in default rates, also by 0.90 percentage points (specification (2)). Similar to the estimates reported by Morse (2015) for the U.S., default rates are on average about 2% among RenrenDai loans (Table 1). [86] Our estimates imply, therefore, that defaults increase by 45% in relative terms, which appears economically very relevant. Here too, we find that the effect is entirely driven by the extensive margin, and disappears once we control for borrower fixed effects. The interpretation is that the increased default rates occur primarily among "new" borrowers, who register on RenrenDai or start borrowing after 2013, and are thus more likely driven by the minimum down-payment increase.

We further find, in Table 8.B (specifications (1)-(2)), that the increase in defaults is mainly associated with borrowers who are home owners. This is consistent with our earlier results, as

[86] In the second half of 2015, there was a wave of defaults on P2P loans across mainland China, with much higher default rates than the 2% average associated with the entire sample ("China's Unregulated P2P Lending Sites are Still Spreading Financial Instability", *China Economic Review* July 28, 2015; "China Imposes Caps on P2P Loans to Curb Shadow-Banking Risks", *Bloomberg News*, August 24, 2016). We are able to observe the increase in defaults in our data; however, given its timing, it has a minimal impact on our estimates around the 2013 increase in minimum down-payment requirements. In particular, the 2015 default wave does not appear to have affected the treated cities in our test differently from the control cities.

home owners are more likely attempting to circumvent the minimum down-payment increase. We combine this test with a further one, in which we estimate an AR(1) model for house price indexes, obtaining house price growth forecasts for the treated cities. We then compute forecast errors, and run separate tests for cities where the forecast proves to be higher/lower than actual house price growth (specifications (3)-(4)). We find that the increase in defaults is estimated more precisely in cities where house price growth underperforms the forecast, but the point estimates are similar across the two groups.

Taken together, the evidence presented in this section and the previous one indicates that: (i) Following the 2013 tightening of LTV caps, P2P borrowing rises abnormally at the treated cities; (ii) P2P lenders do not respond by adjusting their screening procedures, nor do they alter the pricing and duration of their loans in response; and (iii) Default rates among "new" post-2013 borrowers are systematically higher. This suggests that the "lending technology" of the RenrenDai lenders is not flexible enough to induce them to tighten their lending standards in response to the influx of borrowers in the treated cities after November 2013, even though the loans they make turn out to be riskier.

## 3.6. Evidence on the 2015 episode

As explained, in September 2015 a reverse policy intervention is implemented across the country. As part of a broader stimulus package, the minimum down-payment requirements on first and second homes are lowered by 16.7%, from 30% to 25% of the asset's purchase value and the minimum down-payment requirements on second homes are lowered from either 60% or 70% to 40%, in all cities except Beijing, Guangzhou, Sanya, Shanghai, and Shenzhen. Based on the arguments of Section II, this should curb the demand for P2P lending.

Two caveats are in order here, to correctly interpret the tests we are about to present. First, the demand for P2P lending will decrease, under the assumption that the existing credit demand as of September 2015 incorporates a "P2P component" of borrowers who resort to P2P to meet existing down-payment requirements. This appears plausible, based on our findings on the effects of the 2013 policy intervention discussed in the preceding sections. Second, although the 2013 episode provides a relatively "clean" experiment based on the unintended effects of the regulatory intervention, in 2015 the credit authorities may be more aware of the potential role of P2P lending.

Bearing these caveats in mind, we run tests similar to the ones presented in Sections IV and V. First, we examine changes in lending volumes following September 2015, comparing "treated" and "control" cities, where the treatment group includes all Chinese cities with the exception of Beijing, Guangzhou, Sanya, Shanghai, and Shenzhen, which form the control group. The results are illustrated in Table 9.A. Consistent with our expectations, we find that P2P lending drops at the treated cities, mainly driven by the extensive margin. In other words, some borrowers abandon the P2P channel altogether. In economic terms, the effects are similar to the ones presented in Section IV, but with the reverse sign.

Second, we look at lending outcomes, in Table 9.B. Once again, we do not observe large changes in screening: the coefficient estimate in specification (4) on on-site verification, although statistically significant, is economically small at 2.4 percentage points; furthermore, we do not find any significant changes in credit scores, and the magnitude of the coefficient estimate in specification (3) is economically negligible. Given that, following the lowering of down-payment requirement, the P2P lenders should expect *fewer* bad borrowers, there is no reason they should tighten their screening.

We do observe statistically significant changes in the loan terms (pricing and duration), but again they appear economically small. Yield spreads are reduced by about 10 bps, compared to the sample average of nearly 8%; loan duration drops by about 6%, or 1.5 months compared to the sample average of 27 months. Taken together, these findings, as well as those of Sections IV and V, suggest that P2P lenders are generally unresponsive to changes in mortgage LTV caps (and potentially policy interventions in the credit market in general), i.e. they do not condition their lending decisions to the expected "type" of borrower they may face. This is perhaps consistent with the lower sophistication anecdotally associated with P2P lenders, although as we documented RenrenDai lenders have relatively large loan portfolios, suggesting some degree of financial sophistication. On the other hand, this evidence indicates that the benefits of P2P in terms of informal contracting and "proximate knowledge" found by the earlier literature may be limited in this context.

Finally, we do not observe any material changes in delinquencies. Default rates decline by 3.8 percentage points in the treated cities, but the coefficient estimate is at best marginally statistically significant (t-stat: 1.52).

## 3.7. Discussion and policy implications

Since the financial crisis of 2007-2009, evidence has accumulated documenting the negative effects of household leverage, and how high levels of debt exacerbate the business cycle (Lamont and Stein (1999), Mian, Rao, and Sufi (2013), Mian, Sufi, and Verner (2017)). Macroprudential tools have been the focus of much of the debate on how to design policies to contain household leverage (Allen and Carletti (2011), Hanson, Kashyap, and Stein (2011)), and there is evidence showing that they can be effective (Igan and Kang (2011)).

Our findings, on the other hand, point to a vulnerability of LTV caps affecting loans made by traditional credit providers such as banks. Long considered one of the main tools of macroprudential regulation, we provide evidence suggesting that they can be eluded via the P2P credit channel. By themselves, P2P loans are not a threat to financial stability; however, because of the nature of LTV caps, even a relatively small amount of P2P credit can lead to a large mortgage debt.

A policy solution may not be trivial. One possible approach is to broaden the scope of mortgage credit regulation to ratios other than LTV, such as debt-to-income (DTI), which take into account the entire debt position of the prospective borrower; and indeed the literature on macroprudential regulation discusses DTI as a relevant additional tool (Crowe, Dell'Arriccia, Igan, and Rabanal (2012)). That, however, requires setting up a credit registry; and monitoring P2P loans implies collecting information to a level of detail which, to the best of our knowledge, is unprecedented in most developed economies. DTI caps, moreover, may limit households' ability to (efficiently) borrow against future income to smooth consumption over their life cycle. Finally, subjecting P2P platforms to more stringent documentation and transparency requirements risks eroding the very flexibility that makes them a viable business in the first place.

## 3.8. Conclusion

We investigate the capacity of P2P credit to undermine loan-to-value (LTV) caps in mortgage markets. We rely on a novel, hand-collected database containing all loan transactions at RenrenDai, a leading Chinese P2P credit platform, and focus on the increase in 2013 of down-payment requirements on second-home mortgages at several major Chinese cities. This tightening of LTV caps raises the demand for P2P credit by borrowers, who try to circumvent the new down-

payment requirement. Consistent with this argument, P2P loans increase at the treated cities relative to the control cities following the new LTV cap. Importantly, the structure of our data allows us to separate credit demand and supply effects, using a lender × date fixed effects strategy – we are thus able to isolate the capacity of the P2P channel to fuel household debt. We validate this analysis with evidence from a reverse experiment in 2015, when city governments lower minimum down-payment requirements, resulting in a drop in P2P credit demand. In either episode, we find little evidence that P2P lenders adjust their policies in response to the expected characteristics of their borrowers, suggesting that the information benefits of P2P credit that have been observed by part of the literature may be limited. Our results indicate that P2P credit can act as a channel to circumvent LTV caps affecting loans made by traditional credit providers (and potentially other macroprudential tools). The rapid growth of P2P credit in recent years and its largely unregulated and informal nature suggest that a policy solution may not be trivial.

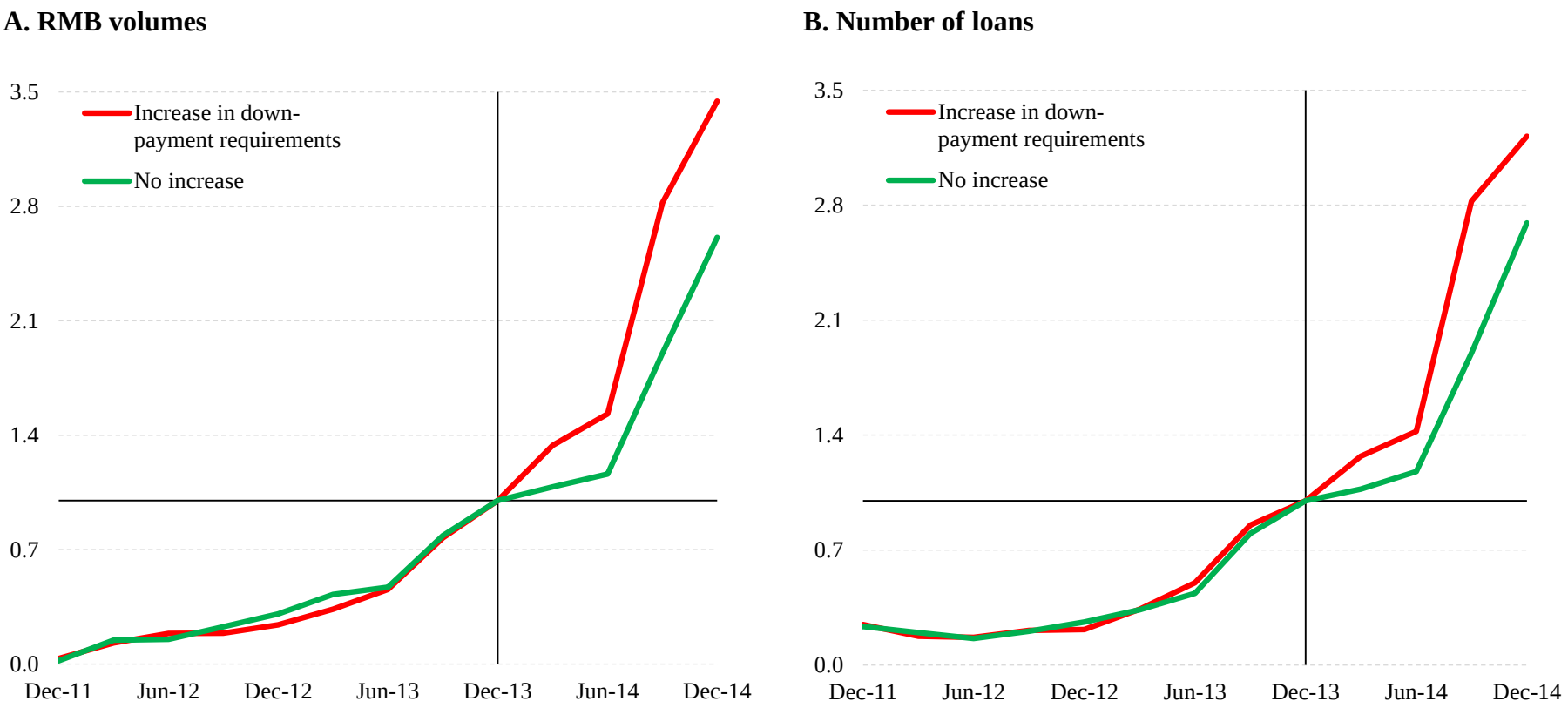


**Figure 1 P2P Loan Applications at RenrenDai around the 2013 Increase in Down-Payment Requirements**

The graphs plot the P2P loan applications on the RenrenDai platform, for treated and control cities, around the 2013 increase in mortgage down-payment requirements. In panel A, the vertical axis reports the city-level RMB loan applications volume per capita, averaged across all treated cities (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Shanghai, Shenyang, Shenzhen, and Wuhan) and control cities (all other Chinese cities with population above 5 million). In panel B, the vertical axis reports the number of loan applications per capita, averaged across treated and control cities. We normalize each series so as to equal 1 on the date of the change in down-payment requirements (the fourth quarter of 2013), such that the vertical axis represents the relative change in P2P loan applications compared to that date. The graph shows that, after the increase in down-payment requirements, the growth in P2P loan applications in the treated cities is higher than in the control cities.

**Table 1 Summary Statistics**

The table reports summary statistics. Panel A describes loan characteristics, panel B borrower characteristics, and panel C lender characteristics. All variables are defined in detail in Appendix A. The sample consists of all loans on the RenrenDai platform, over the period 2011Q1-2015Q2 for borrowers located in metropolitan areas in mainland China with population above 5 million.

| | **Mean** | **St. dev.** | **Min** | **Median** | **Max** | **N** |
|---|---|---|---|---|---|---|
| *A. Loan characteristics* | | | | | | |
| Loan amount (RMB) | 59,674 | 53,816 | 3,000 | 52,900 | 3,000,000 | 107,502 |
| Interest rate (%) | 12.49 | 1.01 | 7.00 | 12.60 | 24.40 | 107,502 |
| Interest rate spread (%) | 7.78 | 1.07 | 2.89 | 7.84 | 19.81 | 107,502 |
| Duration (months) | 27.06 | 9.78 | 1 | 24 | 36 | 107,502 |
| On-site verification (Y/N) | 0.77 | 0.42 | 0 | 1 | 1 | 107,457 |
| Borrower credit score | 171.82 | 29.71 | 0 | 180 | 181 | 107,339 |
| Proportion of months delinquent (%) | 1.96 | 11.35 | 0 | 0 | 100 | 107,502 |
| Default (0/1) | 0.02 | 0.14 | 0 | 0 | 1 | 78,289 |
| *B. Borrower characteristics* | | | | | | |
| Income (monthly RMB) | 11,334 | 13,254 | 0 | 5,000 | 50,000 | 107,494 |
| Age | 37.74 | 8.41 | 23 | 36 | 56 | 107,502 |
| College degree (0/1) | 0.52 | 0.50 | 0 | 1 | 1 | 107,498 |
| Male (0/1) | 0.64 | 0.48 | 0 | 1 | 1 | 107,502 |
| Home owner (0/1) | 0.50 | 0.50 | 0 | 1 | 1 | 107,502 |
| Number of applications since registration | 1.35 | 3.54 | 1 | 1 | 148 | 107,502 |
| Total amount borrowed since registration (RMB) | 66,079 | 99,927 | 3,000 | 53,600 | 9,000,000 | 107,502 |
| *C. Lender characteristics* | | | | | | |
| Portfolio size (RMB) | 387,978 | 485,871 | 4,689 | 289,434 | 4,215,150 | 107,502 |
| Portfolio size (nr. loans) | 234.53 | 156.08 | 4.00 | 199.99 | 1,975 | 107,502 |
| Uplan lending (% of RMB) | 67.18 | 31.26 | 0 | 86.02 | 100 | 107,502 |
| Uplan lending (% of loans made) | 71.94 | 30.49 | 0 | 91.20 | 100 | 107,502 |
| Portfolio concentration (HHI) | 0.007 | 0.019 | 0 | 0.001 | 1 | 107,502 |
| Experience (months since first loan) | 6.86 | 4.31 | 0 | 5.80 | 37 | 107,502 |
| Number of lenders per loan | 44.87 | 55.06 | 1 | 30 | 1,841 | 107,457 |

**Table 2 Comparison of Treatment and Control Groups pre-November 2013**

The table compares the characteristics of borrowers and lenders on loans associated with cities in the treatment (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Shanghai, Shenyang, Shenzhen, and Wuhan) and control groups (all other Chinese cities with over 5 million inhabitants) prior to the November 2013 increase in minimum mortgage down-payment requirements. All variables are defined in detail in the appendix. The column labeled "Treated" reports the average of each characteristic for the treatment group, the column "Control" for the control group, the column "Difference" their difference, and the column "t-statistic" the t-test statistic for the difference, based on standard errors clustered around cities.

| | **Treated** | **Control** | **Difference** | **t-statistic** |
|---|---|---|---|---|
| *A. Borrower characteristics* | | | | |
| Income (RMB) | 11,216 | 11,872 | -656.27 | -0.731 |
| Age | 39.18 | 38.73 | 0.449 | 1.175 |
| College degree (0/1) | 0.51 | 0.45 | 0.06 | 1.695* |
| Male (0/1) | 0.59 | 0.57 | 0.02 | 0.877 |
| Home owner (0/1) | 0.18 | 0.27 | -0.09 | -2.040** |
| Number of applications since registration | 1.51 | 2.06 | -0.56 | -0.974 |
| Total amount borrowed since registration (RMB) | 69,501 | 65,005 | 4,494 | 0.536 |
| *B. Lender characteristics* | | | | |
| Portfolio size (RMB) | 468,649 | 492,152 | -23,503 | 0.833 |
| Portfolio size (nr. loans) | 268.2 | 275.6 | -7.385 | 0.579 |
| Uplan lending (% of RMB) | 68.93 | 71.64 | -2.712 | 0.640 |
| Uplan lending (% of loans made) | 72.76 | 75.57 | -2.805 | 0.655 |
| Portfolio concentration (HHI) | 0.006 | 0.005 | 0.001 | -0.530 |
| Experience (months since first loan) | 4.492 | 4.396 | 0.095 | -0.745 |
| Number of lenders per loan | 33.37 | 33.81 | -0.44 | -0.272 |
| *C. Macroeconomic characteristics* | | | | |
| Province GDP per capita (RMB) | 60,301 | 46,991 | 13,310 | 1.060 |
| Province population (× 10,000) | 5,251 | 6,249 | -998 | -0.649 |
| Province annual GDP per capita growth (%) | 8.16 | 11.20 | -0.03 | -1.336 |
| Province annual population growth (%) | 1.04 | 0.76 | 0.28 | 0.690 |
| Monthly % change in house prices (past 18 months) | 44.3 | 60.1 | -15.8 | 0.543 |
| Household net debt-to-income | -0.745 | -0.422 | -0.323 | -1.299 |
| Real wage index | 1.425 | 1.613 | -0.188 | -0.826 |
| Annual wage growth (%) | 10.7 | 11.0 | 0.3 | 0.261 |
| Unemployment rate (%) | 13.4 | 14.5 | 1.5 | 0.544 |
| RenrenDai penetration (applications per 10,000 inhabitants) | 1.725 | 1.411 | 0.314 | 0.773 |

**Table 3 P2P Lending around the 2013 Increase in Mortgage Down-Payment Requirements: City Level**
The table reports the estimates of:

$$L_{ct} = \alpha + \beta Treated_c + \gamma Post_t + \delta(Treated_c \times Post_t) + \mu' x_{ct} + \varepsilon_{ct}$$

Each observation corresponds to a given city $c$ on a given calendar quarter $t$. The dependent variable is the log-loan amount associated with the aggregate loan applications or actual loan volume in the city. $Treated$ is an indicator variable equal to 1 if the city belongs to the treatment group (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Shanghai, Shenyang, Shenzhen, and Wuhan). $Post$ is an indicator variable equal to 1 over the period followinga change in mortgage down-payment requirements. In specifications (1)-(2), the sample period covers a window of $\pm 1$ year around the down-payment increase; in specifications (3)-(4), $\pm 2$ years. Specifications (1) and (3) focus on loan applications, and specifications (2) and (4) restrict the focus to loans that are actually granted. To control for serial correlation in the standard errors, we time-average and collapse the data (Bertrand, Duflo, and Mullainathan (2004)), and estimate:

$$\Delta L_c = \alpha + \delta Treated_c + \mu' \Delta x_c + \eta_c$$

where $\Delta L_c$ denotes the change in log-loan amount from before to after the change in down-payment requirements. In all specifications, the vector of control variables $x$ includes province GDP and population level and past growth rates, city-level house price % growth over the past 18 months, and yearly real wages and wage growth, and city-level net household debt over income. The standard errors (reported in parentheses) are clustered at the city level. Panel B estimates analogous specifications, where the dependent variable is the (change in) house price growth rate in city $c$, before and after the down-payment requirement increase. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

*A. Credit volumes*

| | ***± 1 year around down-payment requirement increase*** | | ***± 2 years around down-payment requirement increase*** | |
|---|---|---|---|---|
| | **Applications** | **Loans** | **Applications** | **Loans** |
| | (1) | (2) | (3) | (4) |
| $Treated$ | 0.105*** | 0.066* | 0.131*** | 0.074* |
| | (0.035) | (0.037) | (0.040) | (0.039) |
| Controls | Y | Y | Y | Y |
| $R^2$ | 0.54 | 0.49 | 0.60 | 0.54 |
| N | 51 | 51 | 51 | 51 |

*B. House price growth*

| | ***± 1 year around down-payment requirement increase*** | | ***± 2 years around down-payment requirement increase*** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Treated$ | 0.009 | -0.004 | 0.014** | -0.004 |
| | (0.008) | (0.011) | (0.006) | (0.005) |
| Controls | N | Y | N | Y |
| $R^2$ | 0.10 | 0.47 | 0.14 | 0.75 |
| N | 51 | 51 | 51 | 51 |

**Table 4 P2P Lending around the 2013 Increase in Mortgage Down-Payment Requirements: Lender-Borrower Level**

The table reports the estimates of:

$$\Delta L_{lb} = \alpha + \delta Treated_{bc} + \mu' \Delta x_{bc} + \varepsilon_{lb}$$

Each observation corresponds to a given pair borrower $b$-lender $l$. The dependent variable is the change in the natural logarithm of loans made by lender $l$ to borrower $b$ (average after the 2013 increase in down-payment requirements minus average before that). $Treated$ is an indicator variable equal to 1 if borrower $b$ is located in a city $c$ that belongs to the treatment group (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Shanghai, Shenyang, Shenzhen, and Wuhan). Following Bertrand, Duflo, and Mullainathan (2004), the equation is estimated on changes around the down-payment requirement increase, after collapsing and time-averaging the data around the policy intervention. All specifications except (4), include lender fixed effects, corresponding to controlling for a lender-specific intercept before and after the 2013 increase in down-payment requirements. Specifications (1)-(4) focus on loan volumes in the full sample, specification (5) on the sub-sample of borrowers who borrow on RenrenDai both before and after the down-payment increase, and specification (6) on the subset of borrowers who borrow on RenrenDai only before or only after. Province controls include province GDP per capita level and GDP growth over the past 12 months, province population level and population growth over the past 12 months, and the % change of the city house prices in the previous 18 months. Labor market controls include city-level unemployment rate, yearly real wages and wage growth. Household finance controls include city-level household net debt over income. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | **Full Sample** | | | | **Intensive margin** | **Extensive margin** |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Treated$ | 0.020* | 0.025** | 0.025** | 0.034* | -0.001 | 0.025** |
| | (0.012) | (0.012) | (0.012) | (0.019) | (0.013) | (0.012) |
| Province controls | Y | Y | Y | Y | Y | Y |
| Labor market controls | N | Y | Y | Y | Y | Y |
| Household finance controls | N | N | Y | Y | Y | Y |
| Region FE | Y | Y | Y | Y | Y | Y |
| Lender FE | Y | Y | Y | N | Y | Y |
| $R^2$ | 0.39 | 0.39 | 0.39 | 0.03 | 0.58 | 0.38 |
| N | 5,051,602 | 5,051,602 | 5,051,602 | 5,065,546 | 94,542 | 4,938,974 |

**Table 5 Credit Volumes, Borrower Home Ownership, and House Price Growth**

The table reports the estimates of regressions with identical specification as in Table 4, estimated over alternative sub-samples. Specifications (1)-(2) focus on borrower home ownership (Y/N); specifications (3)-(4) on the borrower's city house price growth rate (High – above the median/Low – below the median). The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | **Borrower home owner** | | **Borrower city house price growth** | |
|---|---|---|---|---|
| | **Yes** | **No** | **High** | **Low** |
| | (1) | (2) | (3) | (4) |
| *Treated* | 0.038*** | -0.006 | 0.014* | 0.010 |
| | (0.013) | (0.018) | (0.008) | (0.024) |
| Province controls | Y | Y | Y | Y |
| Labor market controls | Y | Y | Y | Y |
| Household finance controls | Y | Y | Y | Y |
| Region FE | Y | Y | Y | Y |
| Lender FE | Y | Y | Y | Y |
| $R^2$ | 0.37 | 0.40 | 0.38 | 0.40 |
| N | 4,162,218 | 4,255,312 | 4,352,859 | 4,065,767 |

**Table 6 Credit Volumes and Lender Characteristics**

The table reports the estimates of regressions with identical specification as in Table 4, estimated over alternative sub-samples defined by lenders' characteristics. Panel A focuses on whether a loan is made via Uplan or direct peer-to-peer and whether the lender's experience is low or high (below/above the median). Panel B focuses on the size of the lender's portfolio, which we divide into quartiles. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

*A. Lending channel and experience*

| | **Lending channel** | | **Experience** | |
|---|---|---|---|---|
| | **Uplan** | **Direct** | **Low** | **High** |
| | (1) | (2) | (3) | (4) |
| *Treated* | 0.029** | 0.008 | 0.018* | 0.012** |
| | (0.013) | (0.010) | (0.010) | (0.006) |
| Province controls | Y | Y | Y | Y |
| Labor market controls | Y | Y | Y | Y |
| Household finance controls | Y | Y | Y | Y |
| Region FE | Y | Y | Y | Y |
| Lender FE | Y | Y | Y | Y |
| $R^2$ | 0.35 | 0.60 | 0.65 | 0.26 |
| N | 3,990,351 | 1,053,846 | 2,480,133 | 2,445,586 |

*B. Portfolio size*

| | **Quartile** | | | |
|---|---|---|---|---|
| | **Bottom** | **2** | **3** | **Top** |
| | (1) | (2) | (3) | (4) |
| *Treated* | 0.004 | 0.017 | 0.034** | 0.032** |
| | (0.006) | (0.011) | (0.013) | (0.013) |
| Province controls | Y | Y | Y | Y |
| Labor market controls | Y | Y | Y | Y |
| Household finance controls | Y | Y | Y | Y |
| Region FE | Y | Y | Y | Y |
| Lender FE | Y | Y | Y | Y |
| $R^2$ | 0.69 | 0.55 | 0.42 | 0.26 |
| N | 1,172,187 | 1,178,507 | 1,109,200 | 1,549,134 |

**Table 7 P2P Loan Pricing and Screening of the Borrowers around the 2013 Increase in Down-Payment Requirements**

The table reports the estimates of:

$$Y_{bt} = \alpha + \beta Treated_b + \gamma Post_t + \delta(Treated_b \times Post_t) + \mu' x_{bt} + \varepsilon_{bt}$$

Each observation corresponds to a given borrower $b$ on a given calendar date $t$. The dependent variable $Y_{bt}$ is the on-site verification indicator ((1)-(2)), the borrower's credit score ((3)-(4)), the interest rate spread associated with the loan (spec. (5)-(6)), and the natural logarithm of the duration of the loan ((7)-(8)). $Treated$ is an indicator variable equal to 1 if the city belongs to the treatment group (Beijing, Changsha, Guangzhou, Hangzhou, Nanjing, Nanchang, Shanghai, Shenyang, Shenzhen, and Wuhan). $Post$ is an indicator variable equal to 1 over the period following a change in mortgage down-payment requirements (after November 2013 for all treated cities, with the exception of Beijing, where it is equal to 1 following May 2013). In all specifications, the vector of control variables $x$ includes city fixed effects, calendar month fixed effects, administrative region × calendar month fixed effects, city-level house price % growth over the past 18 months, borrower age, income, college degree, gender, number of applications the borrower, total amount borrowed since registration, number of lenders per loan, and yearly macroeconomic controls province GDP and population level and past growth rates, city-level unemployment rate, real wage level and wage growth, and city-level household net debt over income (in these specifications, fixed borrower characteristics and yearly macroeconomic controls are dropped). Specifications (2), (4), (6), and (8) also control also for borrower fixed effects. The standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

| | **On-site verification** | | **Credit score** | | **Spread** | | **Duration** | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $Treated \times Post$ | -0.060 | 0.006 | -0.031 | 0.002 | -0.000 | -0.002 | -0.020 | -0.073 |
| | (0.054) | (0.039) | (0.031) | (0.010) | (0.001) | (0.002) | (0.025) | (0.066) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y |
| City FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Month FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Region × Month FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Borrower FE | N | Y | N | Y | N | Y | N | Y |
| $R^2$ | 0.50 | 0.98 | 0.20 | 0.99 | 0.45 | 0.77 | 0.44 | 0.90 |
| N | 98,601 | 4,111 | 98,601 | 4,111 | 98,601 | 4,111 | 98,601 | 4,111 |

**Table 8 P2P Loan Performance following the 2013 Increase in Down-Payment Requirements**

The table reports the estimates regressions analogous to Table 7. In panel A, the dependent variable is delinquency, the proportion of months during the borrowing period in which the borrower is delinquent (spec. (1)-(2)), or a default indicator (spec. (3)-(4)). In panel B, the dependent variable is delinquency, and the sample is split between loans to borrowers who own a home or not (spec. (1)-(2)), as well as between loans to borrowers located in cities with high/low (above/below the median) ex post house price forecast error (spec. (3)-(4)). The vector of control variables $x$ is the same as in the regressions of Table 7. Specifications (2) and (4) in Panel A also control also for borrower fixed effects. In both panels and all specifications, the standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

*A. Full sample*

| | **Delinquency** | | **Default** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Treated \times Post$ | 0.009* | -0.027 | 0.009* | -0.056 |
| | (0.004) | (0.040) | (0.004) | (0.042) |
| Controls | Y | Y | Y | Y |
| City FE | Y | Y | Y | Y |
| Month FE | Y | Y | Y | Y |
| Region × Month FE | Y | Y | Y | Y |
| Borrower FE | N | Y | N | Y |
| $R^2$ | 0.20 | 0.65 | 0.12 | 0.52 |
| N | 98,601 | 4,111 | 70,469 | 3,547 |

*B. Borrower home ownership and house price forecast error*

| | **Borrower home owner** | | **Borrower city house price forecast error** | |
|---|---|---|---|---|
| | **Yes** | **No** | **High** | **Low** |
| | (1) | (2) | (3) | (4) |
| $Treated \times Post$ | 0.010* | 0.006 | 0.008* | 0.008 |
| | (0.005) | (0.005) | (0.004) | (0.090) |
| Controls | Y | Y | Y | Y |
| City FE | Y | Y | Y | Y |
| Month FE | Y | Y | Y | Y |
| Region × Month FE | Y | Y | Y | Y |
| $R^2$ | 0.21 | 0.21 | 0.21 | 0.21 |
| N | 78,304 | 85,553 | 83,920 | 79,937 |

**Table 9 P2P Lending around the 2015 Decrease in Down-Payment Requirements**

Panel A reports the estimates of regressions analogous to Table 4, estimated around the September 2015 decrease in down-payment requirements. In this case, the $Treated$ indicator equals 1 for all Chinese cities with at least 5 million inhabitants except Beijing, Guangzhou, Shanya, Shanghai, and Shenzhen. Panel B reports the estimates of regressions analogous to Tables 7 and 8, estimated again around the September 2015 decrease in down-payment requirements. The control variables are the same as in the regressions of Table 7. Specifications (2) and (4) in Panel A also control also for borrower fixed effects. In all panels and specifications, the standard errors (reported in parentheses) are clustered at the city level. The symbols *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

*A. Credit volumes*

| | **Full Sample** | | **Intensive margin** | **Extensive margin** |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $Treated$ | -0.048* | -0.027** | -0.017 | -0.027** |
| | (0.028) | (0.011) | (0.013) | (0.012) |
| Controls | Y | Y | Y | Y |
| Region FE | Y | Y | Y | Y |
| Lender FE | N | Y | Y | Y |
| $R^2$ | 0.012 | 0.390 | 0.444 | 0.398 |
| N | 14,367,497 | 13,960,421 | 313,499 | 13,589,210 |

*B. Loan pricing, screening of the borrowers, and loan performance*

| | **On-site verification** | **Credit score** | **Spread** | **Duration** | **Delinquency** | **Default** |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $Treated \times Post$ | -0.024** | -0.088 | -0.001*** | -0.064*** | 0.006 | -0.038 |
| | (0.011) | (0.058) | (0.000) | (0.019) | (0.005) | (0.025) |
| Controls | Y | Y | Y | Y | Y | Y |
| City FE | Y | Y | Y | Y | Y | Y |
| Month FE | Y | Y | Y | Y | Y | Y |
| Region × Month FE | Y | Y | Y | Y | Y | Y |
| $R^2$ | 0.36 | 0.48 | 0.47 | 0.47 | 0.15 | 0.09 |
| N | 182,680 | 184,417 | 184,433 | 184,433 | 184,433 | 59,730 |

# Appendix 3

## Variable Definitions

| Variable | Definition |
|---|---|
| *A. Loan characteristics* | |
| *Loan amount (RMB)* | Amount of the loan in RMB. |
| *Interest rate (%)* | Annual interest rate applied to the loan. |
| *Interest rate spread (%)* | Annual interest rate minus the corresponding one-year Shibor rate. |
| *Duration (months)* | Maturity of the loan, expressed in number of months. |
| *On-site verification (Y/N)* | Indicator variable that takes the value of 1 if an officer from RenrenDai verified that the information provided by the borrower on the internet platform is true, by visiting the borrower at her stated address. |
| *Credit score* | Credit score assigned to the borrower by RenrenDai. |
| *Proportion of Months Delinquent (%)* | The proportion of months, over the loan's life, during which the borrower is delinquent. A borrower is delinquent if she misses or delays the monthly payment of the interest and/or the monthly repayment of the principal. |
| *Default (0/1)* | Indicator variable that takes the value of 1 if a loan is declared in default and 0 otherwise. |
| *B. Borrower characteristics* | |
| *Income (RMB)* | Borrower's monthly income at the origination of the loan. RenrenDai provides this information in brackets: between 0 and 1,000, between 1,001 and 2,000, between 2,001 and 5,000, between 5,001 and 10,000, between 10,001 and 20,000, between 20,001 and 50,000, and above 50,000 RMB. |
| *Age* | Age of the borrower at the origination of the loan. |
| *College degree (0/1)* | Indicator variable that takes the value of 1 if the borrower has a college degree or higher education level. |
| *Male (0/1)* | Indicator variable that takes the value of 1 if the borrower is a male. |
| *Home Owner (0/1)* | Indicator variable that takes the value of 1 if the borrower owns a house and 0 otherwise. |
| *Number of applications since registration* | Number of loan applications, at the time of the loan origination, made by the borrower since her registration in RenrenDai. |
| *Total Amount Borrowed since registration* | Total RMB borrowed by the borrower on Renredai at the time of the loan origination since her registration |
| *C. Lender characteristics* | |
| *Portfolio size (RMB)* | Size of lenders's portfolio, measured in RMB. |
| *Portfolio size (nr. loans)* | Size of lender's portfolio, measured in number of loans. |
| *Uplan lending (% of RMB)* | % of the lender's portfolio (measured in RMB) invested via Uplan. |

| | |
|---|---|
| *Uplan lending (% of loans made)* | % of the lender's portfolio (measured in number of loans) invested via Uplan. |
| *Portfolio concentration (HHI)* | Concentration of the lenders' portfolio. Concentration is measured with a Herfindahl-Hirschman index (HHI), based on the relative proportion of each loan with respect to the total size of the lender's portfolio. |
| *Experience (months since first loan)* | Experience of the lender, measured as a the number of months between the origination of the loan and the first loan made by the lender on Renrendai. |
| *Number of Lenders per loan* | Number of lenders funding a particular loan issue on Renrendai |
| *D. Macroeconomic variables* | |
| *Province GDP per capita* | GDP per capita of the province where the borrower's city is located, retrieved from the CSMAR database. |
| *Province population* | Population of the province where the borrower's city is located, retrieved from the CSMAR database. |
| *Province annual GDP per capita growth* | Annual GDP per capita growth of the province where the borrower's city is located, retrieved from the CSMAR database. |
| *Province annual population growth (%)* | Annual population growth of the province where the borrower's city is located, retrieved from the CSMAR database. |
| *Monthly % change in house prices (past 18 months)* | Average growth of house prices in the city during the past 18 months, retrieved from the China Index Academy databank. |
| *Household net debt to income* | Total city household debt minus total city households bank deposits divided by city GDP, retrieved from the CSMAR database. |
| *Real wage index* | Average wage per worker in the city divided by the city's CPI (base, Shanghai in November 2013), retrieved from the CSMAR database. |
| *Annual nominal wage growth* | Average growth of nominal wages per workers in the city, retrieved from the CSMAR database. |
| *Unemployment rate* | Number of unemployed individuals in the city divided by the city labor force, retrieved from the CSMAR database. |
| *RenrenDai penetration* | Number of loan applications per city in a given year divided by city population (in thousands) in the same year. |

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